Optimal Incentives and Distributive Justice in Competitive Environments

Dissertation
submitted to the
Faculty of Business, Economics and Informatics
of the University of Zurich

to obtain the degree of
Doktor der Wirtschaftswissenschaften, Dr. oec.
(corresponds to Doctor of Philosophy, PhD)

presented by

Arnd Heinrich Klein
from Germany

approved in April 2015 at the request of
Prof. Dr. Armin Schmutzler
Prof. Dr. Roberto Weber

The Faculty of Business, Economics and Informatics of the University of Zurich hereby authorizes the printing of this dissertation, without indicating an opinion of the views expressed in the work.

Zurich, 01.04.2015

Chairman of the Doctoral Board: Prof. Dr. Josef Zweimüller *to my parents and Andrea*

Acknowledgements

First and foremost, I want to thank my supervisor Professor Armin Schmutzler for his excellent academic guidance, for his strong support, and for giving me the freedom to turn my doctoral studies into an exciting and enriching experience. I would also like to thank Professor Roberto Weber for taking over the co-supervision and for his excellent advice on my experimental projects.

On this occasion, I wish to thank Professor Friedrich Breyer (University of Konstanz), Professor John Morgan (UC Berkeley) and Professor Wesley Wilson (University of Oregon) for their advice and support at different stages of my economic education, which opened up new opportunities and had significant impact on my academic career.

I would also like to thank my former colleagues and fellow PhD students in Zurich and Berkeley, Abhay Aneja, Carlos Avenancio, Dr. Andreas Beerli, Jean-Michel Benkert, Dr. Lea Cassar, Dr. Donja Darai, Dr. Patricia Feubli, Dr. Silvia Grätz, Ani Harutyunyan, Dr. Andreas Hefti, Steve Heinke, Ronald Indergand, Dr. Stefan Jönsson, Dr. Jan-Aaron Klaassen, Igor Letina, Professor Nick Netzer, Philippe Ruh, Dr. Frédéric Schneider, Dr. Sabrina Studer and Thomas Wüthrich, for our inspiring discussions and the amazing time we spent with each other.

I wish to thank my parents Karin and Dr. Martin Klein and my siblings Hedda Herdis Klein, Dr. Anne Karin Klein and Dr. Dr. Karl Martin Klein for their continuous encouragement and for being such a great role model in how to master my doctoral studies. Lastly, I am deeply grateful to Andrea Portmann for believing in me and giving me confidence.

Contents

Introduction

This thesis analyzes optimal incentives and distributive justice in competitive environments. It consists of three essays. The first two essays investigate theoretically and empirically the optimal design of incentives in dynamic tournaments. The third essay analyzes empirically how personal income earned in the competition with others changes individuals' perceptions of what is a socially just distribution of this income.

The first two essays focus on tournaments, a type of competitive incentive mechanism. In tournaments, a principal induces efforts among a group of agents by rewarding the best-performing agent with a monetary or non-monetary prize. The use of such incentives, for instance in the form of bonuses or promotions, is widespread within companies. Furthermore, tournaments play a crucial role in the assignment of research grants or in incentivizing the development of new ideas. An important feature of tournaments is that they are often repeated over time. For example, firms usually operate over several years, making their workers compete against each other multiple times. This generates a series of performance signals among the employees. In the design of incentives for effort provision, this dynamic nature of tournaments raises at least three questions: (1) How should principals spread the rewards over the different periods? (2) Which weight should principals give to recent performance relative to performance in the more distant past? And (3) to which extent should the principal reveal the results of past performance measurements to the agents?

The first essay addresses these questions in a game-theoretic framework. The analysis relies on a dynamic two-stage tournament and allows for different objectives of the principal as well as for general effort cost functions and distributions of noise in the performance signals. By simultaneously considering information revelation, the spread of prizes and performance weights as design tools of the principal, the essay provides several new results and generalizes a few existing results. The optimal incentive policy has the following properties: First, the revelation policy depends exclusively on properties of the effort cost function. Second, the principal always puts a positive weight on first-period performance in the second period. Third, the size of the optimal weight and the optimal prizes depend on properties of the observation noise. In particular, the principal sets no first-period prize unless the performance observations in period one are considerably more precise than in period two. The analysis further reveals that the gains from good design are quantitatively important: For a particular parameterization of the general model with normally distributed observation noise and quadratic effort costs, the expected effort is at least 40% higher when a principal chooses prizes and weights optimally than when the principal uses two equal and independent tournaments in both periods.

The second essay uses a laboratory experiment to test whether the optimal policy identified in the first essay increases efforts as predicted in an environment with real subjects. To derive the optimal policy, the second essay also relies on a normal-quadratic parameterization of the general model. The experiment introduced the optimal policy stepwise: It first implemented the optimal weight, and then also the optimal prize spread. It turns out that only the policy that implements both the optimal weight and the optimal prize spread increases efforts, while its effect is smaller than predicted. Nevertheless, all policy adjustments affect the behavior of the participants and change the distribution of efforts across periods. The directions of these changes correspond widely to the predictions of the theoretical model. Overall, there is significant over-expenditure compared to the theoretical predictions and a high degree of heterogeneity in behavior across participants. Measures of individual attributes from a pre-experimental questionnaire completed eight days ahead of the laboratory experiment explain parts of this heterogeneity. In particular, the results suggest that one explanation for the small effect of the optimal policy, which generates a high degree of ex-post income inequality, is that it induces particularly low efforts among prosocial participants.

The first two essays generate insights that are relevant to the design of competitive incentive systems in practice. Clearly, the precise nature of the optimal incentives for a less stylized setting – i.e., with more periods and agents – is likely to differ from the optimal policy derived in the first essay. Nevertheless, the theoretical and empirical evidence provided in both essays jointly suggests that the timing of incentives matters: The weight of past performance and the spread of prize money across periods affect individual behavior. They are, therefore, important parameters to consider when designing competitive incentives in a repeated setting. This highlights the relevance of theoretical arguments, as provided by the first essay, for the design of incentive systems. However, the results suggest as well that practitioners should not blindly rely on guidance from simple material utility models in the design of competitive incentives. In fact, one interpretation of the results of the second essay is that practitioners should examine competitive incentives also regarding their distributive effects and potential consequences for workers with social preferences. Failing to do so may not only lead to wrong predictions of the effects of policy adjustments. It may as well lead to policies which are less efficient than possible.

The third essay takes the existence and design of such competitive incentive systems as given and asks whether the income that individuals earn in competitive interactions with others affects their perception of what is a socially just distribution of this income. The question is motivated by the well-established empirical finding of a negative relationship between individuals' income and their support for redistributive public policies. A common explanation for this pattern is self-interest: While high-income individuals want less redistribution to avoid high taxes, low-income individuals want to benefit from transfers and thus support more redistribution. However, empirical evidence suggests that support for redistribution is not only driven by self-interest, but also by individuals' *fairness views*, i.e., their perceptions of what is a just, or fair, distribution of income in society. Therefore, the negative relationship between support for redistribution and income may as well be due to systematically different views of rich and poor individuals on what is a fair income distribution.

To test this hypothesis, the third essay analyzes experimentally if individuals' income, and how this income is generated, changes their fairness views. The experiment consisted of an income generation phase and a distribution phase. In the income generation phase, participants received a high or a low income either through luck or through an effort-based tournament. In the distribution phase, a subset of subjects was asked to make distributive decisions over the incomes of two other pairs of subjects – one pair in which income differences were due to luck, and one in which income differences were due to effort. Strategic behavior that favors self-interest was, therefore, ruled out. It turns out that low-income individuals redistribute significantly more than high-income individuals when the source of income differences is the same as the one they experienced themselves. That is, when inequalities are due to luck (effort), an individual who received a low income by lack of luck (effort) redistributes significantly more than an individual who received a high income by luck (effort). The effect remains unchanged when controlling for individual performance in the effort-based tournament, suggesting that self-selection into different income levels does not drive the results. This implies that personal income, and how the latter is generated, has a causal effect on individuals' fairness views. Further analysis shows that an explanation for this result is a self-serving bias in the attribution of responsibility over an outcome: Compared to low-income individuals, high-income individuals tend to believe more that their outcome is the result of effort rather than of luck.

The third essay has important implications for our understanding of how societies think about redistribution. The results suggest that personal income changes individuals' views about a fair distribution of income in society. In fact, personal income seems to increase the acceptance of income differences. This provides an explanation for the negative relationship between support for redistribution and income that goes beyond the channel of self-interest. Furthermore, it implies that the conflict between rich and poor in the debate about redistribution is not only a battle of personal interests, but also of different ideologies. This difference in ideologies is such that it increases the discrepancy in preferences for redistribution that is already caused by selfish motives. Therefore, there will be disagreement between rich and poor about income redistribution even if people are able to abstract from their own personal stake in this redistribution. The results further imply that the differences in ideologies between rich and poor are also the result of different individual outcomes in the process of income generation. As a consequence, an increase in income inequality is likely to boost the polarization in political preferences within society, which makes it harder to reach a consensus about the appropriate level of redistribution in the long run.

Chapter 1

Optimal Effort Incentives in Dynamic Tournaments*

joint with Armin Schmutzler

1.1 Introduction

In many contexts, groups of economic agents supply efforts repeatedly, thereby giving rise to sequences of performance signals that principals can use to reward efforts. First, most organizations assess their employees' performance regularly. This performance information plays a crucial role for decisions on bonus payments, promotion and tenure. Second, in many arms-length relationships, buyers repeatedly procure goods and services from the same pool of suppliers. They can use past experience with these suppliers as a basis for the conditions of future interactions. Third, school and university teachers repeatedly observe the performance of students in their classes and can decide how to use this information for final grades.

Motivated by these real-world situations, we analyze the incentive effects of different approaches to rewarding repeated performance. Specifically, we ask the following questions:

1. How often should principals reward agents for good achievements? Should there be frequent small rewards or rare large rewards?

*A revised version of this chapter is accepted for publication in *Games and Economic Behavior*: Klein, A. H. & Schmutzler, A. (2016). Optimal Effort Incentives in Dynamic Tournaments. http://dx.doi.org/10.1016/j.geb.2016.02.006.

2. Which weight should principals give to recent performance relative to performance in the more distant past?

3. To which extent should the principal reveal the results of past performance measurement to the agents?

We answer these questions for dynamic tournaments. Tournaments are often used instead of contracts which condition explicitly and exclusively on each agent's own performance. In particular, organizations indeed provide incentives with promotion tournaments.[1]

Specifically, we consider a two-period tournament with two risk-neutral agents with identical and known abilities. To see the incentive effects of such tournaments most clearly, we abstract from the important issue of selecting the agent with the highest innate ability for a particular task. The principal chooses an incentive system, consisting of (i) the distribution of the prize money across the two periods, (ii) the weight of first-period performance in the second tournament and (iii) the information revelation policy.

After observing the policy, the agents choose effort levels in each period. The principal observes the performance of each agent, which is a noisy measure of effort. In period 1, she awards the prize (if any) to the agent with the higher performance. Under a full revelation policy, she communicates the performance of both agents in the first period. Under a no revelation policy, she neither communicates performance, nor who the winner was. In period 2, the agents choose efforts again. The principal then allocates the second-period prize to the agent for whom the weighted sum of first- and second-period performance is highest.[2]

In line with the existing literature, we consider the case that a principal regards efforts in different periods and by different agents as perfect substitutes and thus maximizes total effort. Contrary to most of the existing literature, we also analyze the optimal policy for a principal who regards efforts in different periods as imperfect substitutes and wants to balance them across periods.[3] We believe this is important, because excessively low efforts in some period may cause large harm, which cannot even be compensated by an extremely large effort in other periods.

[1] A well-known argument for tournaments is that they are more credible because they are less prone to manipulation by the principal than contracts that depend explicitly on the details of performance: When performance is not verifiable, a principal may claim that performance was low to save on performance pay. Tournaments reduce this incentive, because the total payments to the agents are independent of performance.

[2] In the no revelation case, the game is static. The model thus becomes a special case of a multi-battle contest where agents compete simultaneously in a multiplicity of dimensions (see, e.g., Franckx et al., 2004; Clark & Konrad, 2007; Kovenock & Roberson, 2010). However, the dynamic cross-period effects which occur under our full revelation regime are totally absent in these papers.

[3] Specifically, she maximizes the product of first- and second-period efforts, or equivalently, the sum of the logarithms. Aoyagi (2010) also allows for more general objectives than maximizing total efforts.

Apart from allowing for imperfect intertemporal effort substitution, our approach differs from previous literature in three ways. First, we simultaneously consider information revelation, the prize distribution and performance weights as design tools of the principal. Second, we include the possibility that the distributions of the first- and second-period performance measures differ – e.g. in their precision – reflecting heterogeneity of tasks across periods. Third, we allow different cost functions across periods.

Our contribution is threefold. First, we generalize existing results on information revelation. Previous analysis has shown for special cases that expected total efforts are lower with revelation than without when marginal effort costs are convex, and conversely for concave marginal effort costs (see Section 1.2). We show that this result holds for perfect and imperfect substitutes, and for arbitrary first-period prizes and performance weights.

Second, we clarify the relation between first-period prizes and first-period performance weights as incentives for first-period efforts. For both revelation policies and for perfect as well as imperfect substitutes, the optimal first-period prize is positive only if the distribution of the first-period observation error difference is very precise, that is, highly concentrated near zero. We then show that for quadratic cost functions and normally distributed observation errors, this condition is never satisfied. Even with more general distributional assumptions, the scope for using first-period prizes is limited: For imperfect substitutes and quadratic cost functions, the optimal first-period prize is never higher than the second-period prize.

Whereas the optimal first-period prize is typically zero, the optimal weight of first-period performance in the second-period tournament is strictly positive for both revelation policies, general cost functions and error distributions. The optimal weight is higher the lower the adverse effect of increasing the first-period weight on future competitive intensity is. For quadratic cost functions, normally distributed observation errors and perfect (imperfect) substitutes, the optimal weight is the ratio of the variances (standard deviations) of second-period and first-period observation error differences.

Third, we show that the potential gains from good design are quantitatively important. In the normal-quadratic example, the expected effort is at least 40% higher when a principal chooses prizes and weights optimally than when she distributes the prize money evenly across both periods without giving weight to first-period performance in the second-period tournament.

The organization of the study is as follows. Section 1.2 discusses related literature. In Section 1.3, we introduce the model. In Section 1.4, we analyze the behavior of agents for given policies. Section 1.5 characterizes the optimal policy. Section 1.6 interprets and sharpens our results in a normal-quadratic example. Section 1.7 concludes.

1.2 Relation to the literature

In this study, we focus on the optimal design of multi-period rank-order tournaments, in particular, on feedback policy, prize structure and weight of past performance.[4,5] The only paper we are aware of that simultaneously analyzes these three design dimensions is Gershkov and Perry (2009). However, their set-up differs substantially from ours. Most importantly, after period one, the principal merely knows whether there is a tie (arising with positive probability) or whether one of the agents has performed better (and, if so, which agent); there is no information on the size of the lead. In many contexts, such a coarseness of the information structure appears to be appropriate. However, in other contexts, the principal can collect and communicate information that provides the agents with a clear picture of how much their performance differs from the performance of others. This information will typically not be verifiable in a court, but for our purposes it is sufficient that the principal and the agents share a common understanding of the relation between promotion chances and the information communicated about the agents' relative positions.[6] Also, Gershkov and Perry (2009) assume that the relation between winning probabilities and efforts is the same in both periods, while we allow for differences in the error structure. Finally, they only focus on maximization of total effort.

We mention in passing the substantial literature analyzing agent behavior in repeated tournaments without addressing optimal design. Several of these papers allow for effects of first-period play on the second period that are determined by technology rather than, as in our case, by the principal.[7] Moreover, some papers study two-period contests (rank-order, all-pay and Tullock, respectively) where the total effort in the two periods determines the winner of a final prize.[8,9]

---

[4]Nitzan (1994) and Konrad (2009) provide surveys of the literature on tournaments.

[5]A broadly related literature analyzes dynamic principal-agent relationships with moral hazard in a non-competitive setting. Lewis and Sappington (1997) examine how current incentives should optimally depend on past performance. Hansen (2013) and Chen and Chiu (2013) deal with the optimal revelation policy. For reasons of space, however, we will focus on studies that deal with repeated contests.

[6]With verifiable information, the principal could contract directly on efforts, and there would be no need to use tournaments.

[7]See Baik and Lee (2000), Schmitt et al. (2004), Grossmann and Dietl (2009) and Grossmann (2011).

[8]See Yildirim (2005) for Tullock contests and Hirata (2014) for all-pay auctions. Casas-Arce and Martínez-Jerez (2009) consider a related rank-order tournament where all agents whose total performance is higher than a certain threshold win a prize.

[9]More broadly related, several papers analyze the agents' behavior in a sequence of contests where there is a prize for winning each contest, and an overall prize to the agent who is the first to win a certain number of contests. Examples are Konrad and Kovenock (2009) and Krumer (2013). The model of Sela (2011) is similar, the difference being that there is no prize for winning a single contest.

1.2.1 Performance revelation

Several papers analyze the effect of interim performance revelation on efforts in dynamic tournaments. In a setting similar to ours, Aoyagi (2010) shows that expected effort is higher with information revelation than without if and only if marginal effort costs are concave.[10] Unlike in this study, there is only one prize, and first- and second-period weights are the same. We endogenize these assumptions by providing conditions under which the principal optimally chooses the prizes and weights in this way. Moreover, we show that the optimal revelation policy has the same features when these assumptions do not hold.

Ederer (2010) introduces incomplete information about ability. The results are equivalent to those of Aoyagi (2010) if ability is non-complementary to effort. If efforts and ability are complementary, it is possible that information revelation leads to higher expected efforts than no revelation even with quadratic effort costs.[11,12]

1.2.2 The weight of past performance

Several authors ask whether there should be a bias towards the first-period winner in the second period of a multi-period contest (Meyer, 1992; Harbaugh & Ridlon, 2010; Ridlon & Shin, 2013). Meyer (1992) considers a setting similar to our case with information revelation and a single prize, but with risk-averse agents. She shows that the cost-minimizing choice of an effort vector requires a bias towards the first-period winner.[13] Our analysis shows that the argument for giving a headstart also holds when the first-period prize is much higher than the second-period prize, when intertemporal effort substitution is imperfect and when there is no information revelation. Finally, we provide results on the determinants of the size of the bias.[14]

---

[10] Aoyagi (2010) is quite general with respect to the objective of the principal, and he allows for partial revelation. Denter and Sisak (2013) show that effort may increase with revelation if marginal efforts are concave. They use their set-up to analyze the effect of polls on political campaign spending, allowing for an initial asymmetry before the beginning of the first period.

[11] Using a similar framework, Ederer and Fehr (2007) and Marinovic (2014) study the issue of credibility of the performance feedback.

[12] Other papers address the revelation policy in dynamic tournaments under very different assumptions. For example, Arbatskaya and Mialon (2012) analyze a lottery contest where first- and second-period efforts are complements in affecting the probability of winning. They find that revelation of first-period efforts decreases total efforts. Goltsman and Mukherjee (2011) consider a contest in which the agents either succeed or fail, and the prize is given to the agent who succeeded more often. The optimal policy reveals performance only if both agents fail. Finally, Zhang and Wang (2009) consider revelation policies in dynamic all-pay auctions with elimination.

[13] Ridlon and Shin (2013) show for a Tullock contest that an analogous result still holds for small asymmetries in the abilities of agents. However, if the asymmetry is high, favoring the first-period loser is optimal. In the dynamic all-pay auction of Harbaugh and Ridlon (2010), favoring the first-period loser is always optimal.

[14] Contrary to us, Meyer (1992) assumes that the size of the bias is fixed ex ante rather than a function of the performance difference in period 1.

1.2.3 Distribution of prize money

A small number of papers derives the optimal distribution of prize money across periods when there is an exogenously given technological link between the first and the second period, creating an asymmetry between the agents in the second period. The effects of such links are similar to those of a positive weight of past performance in the assignment of the second-period prize. Contrary to us, the authors focus on Tullock contests. For example, in Möller (2012), the prize money received in the first period does not yield direct utility to the agents, but reduces their effort costs in the second period. Under some circumstances, the optimal policy requires a positive prize both for the winner and for the loser in the first period.[15] In Clark et al. (2012), the winner in the first period may have lower effort costs in the second period. The effort-maximizing prize structure is to give only a second-period prize. In Clark and Nilssen (2013), second-period effort costs fall with first-period effort. The authors provide conditions under which it is optimal to pay more than half of the total prize money in the second period.[16] Apart from the obvious difference in the structure of the contest, these papers do not analyze revelation policies, nor do they allow for imperfect substitutes.

Some papers derive the optimal distribution of prize money across stages in a two-period elimination tournament, where only the winners of the current period compete again in the next period. A seminal paper is Moldovanu and Sela (2006). Because elimination tournaments have a very different structure than our model, the results are difficult to compare to ours.

1.3 The model

We consider a class of two-stage rank-order tournaments. Given a fixed budget $W > 0$, a principal chooses an *incentive system* $\mathcal{I}$, which is a tuple $(\eta, W_1, \rho) \in \mathbb{R} \times [0, W] \times \{0, 1\}$ to be explained below. For given $\mathcal{I}$, agents $i \in \{1, 2\}$ choose effort levels $e_{it} \geq 0$ ($t \in \{1, 2\}$).[17] The effort cost function $K_{it}(e_{it})$ has the following properties:

Assumption 1.1. *K_{it} is independent of i and differentiable three times. It satisfies $K'_{it} > 0$, $K''_{it} > 0$, $K_{it}(0) = K'_{it}(0) = 0$. $K'''_{it}(e_{it}) \geq 0$ or $K'''_{it}(e_{it}) \leq 0$ must hold globally.*

[15] Since agents are initially symmetric, unequal prizes in the first period yield an asymmetry in the second period through their effect on second-period effort costs. This result is therefore similar to a positive weight on past performance in our setting.

[16] We have a similar result in the case of imperfect substitutes, but for very different reasons (see Proposition 1.7).

[17] In the following, the use of i and/or j as an index always implies $i, j \in \{1, 2\}$ and $i \neq j$.

Thus, we can write $K_t \equiv K_{it}$. Note that we allow first- and second-period tasks to differ with respect to effort costs. This reflects the idea that the efforts in the two periods may be of very different types. Employees or suppliers may have to carry out different tasks in different periods; students learn different kinds of material in different phases of their education. Therefore, effort costs may differ across tasks.

The agents maximize expected utility and are risk-neutral. Their utility is additively separable in period-specific income and costs. At the end of each period t, the principal observes performance, which is an imperfect measure $s_{it} = e_{it} + \varepsilon_{it}$ of effort. The error term ε_{it} is independently distributed across agents and periods. In each period, the error distribution is the same for agent 1 as for agent 2. However, the error distribution in period 1 may differ from the one in period 2. This captures the notion that tasks in different periods may also differ in terms of how easy it is to monitor effort.[18]

Based on the first-period performance, the principal awards the first-period prize W_1 to agent i if $s_{i1} > s_{j1}$. Furthermore, agent i receives the second-period prize $W_2 = W - W_1$ if $s_{i2} + \eta s_{i1} > s_{j2} + \eta s_{j1}$.[19] The principal's choice of the first-period weight $\eta \in \mathbb{R}$ thus determines the influence of past performance on the chance of winning in the second period.

Under a *full-revelation policy* ($\rho = 1$), the principal communicates the measured performance of both players to the agents before they choose their second-period efforts. In practice, the principal will typically not communicate a concrete number. Instead, she may communicate whatever relevant information she has to the agents, thereby creating a common understanding about their relative performance.[20] Under a *no-revelation policy* ($\rho = 0$), the principal does not communicate the performance assessment. She does not even communicate who won the first-period prize and distributes both prizes at the end of period 2.

The following notation is helpful to describe the solution of the game.

Definition 1.1. The *error difference* of player i in period t ($t = 1, 2$) is $\Delta\varepsilon_{it} = \varepsilon_{it} - \varepsilon_{jt}$, his *relative first-period performance* is $\Delta s_{i1} = s_{i1} - s_{j1} = \Delta e_{i1} + \Delta\varepsilon_{it}$, where $\Delta e_{it} = e_{it} - e_{jt}$.

Clearly, $\Delta e_{it} = -\Delta e_{jt}$, $\Delta\varepsilon_{it} = -\Delta\varepsilon_{jt}$, $\Delta s_{i1} = -\Delta s_{j1}$. We make the following assumption on the error distributions:

---

[18] In a non-tournament setting, Ke et al. (2014) show that organizations optimally hire workers into easy-to-monitor jobs with low effort costs and then promote them into difficult-to-monitor jobs with high (marginal and absolute) effort costs. In our setting, this would correspond to $\sigma_1 < \sigma_2$ and $K_1(e) < K_2(e)$, $K_1'(e) < K_2'(e)$.

[19] In each period, in case of a tie, the principal assigns the prize to each agent with probability one half.

[20] As we will see, second-period efforts depend negatively on the absolute value of the performance difference in the first period. Hence, the principal has an incentive to always report equal performances. This problem becomes negligible if the principal leaves the communication to disinterested parties from within or outside the organization.

Assumption 1.2. $\Delta\varepsilon_{it}$ *is distributed as* $F_t(s)$ *on* $\mathbb{R}$ *with a symmetric, single-peaked, strictly positive and continuously differentiable density* $f_t(s)$*.*

This implies $f_t(s) = f_t(-s)$, $f_t'(s) = -f_t'(-s)$ and $E(\Delta\varepsilon_{it}) = 0$.[21] For some results, we assume that the cost functions are quadratic:

Assumption 1.3. *The cost function is* $K_t(e_{it}) = \frac{k_t}{2}(e_{it})^2$ *with* $k_t > 0$*.*

We assume that, given a fixed prize budget, the principal's payoff is increasing in efforts, where the efforts of different agents within periods are perfect substitutes for the principal. We allow first- and second-period efforts to be either perfect or imperfect substitutes. For *perfect substitutes*, the principal chooses the incentive system so as to maximize expected total efforts. For *imperfect substitutes*, she maximizes the expected product of first- and second-period efforts. This corresponds to a complementarity between first- and second-period efforts, making it desirable to have similar efforts in both periods.

1.4 Behavior of the agents

We first analyze the equilibrium behavior of agents for given incentive system. The following simple result is mentioned without proof.

Lemma 1.1.

(i) The conditional probability that $s_{i1} > s_{j1}$ *given* e_{i1} *and* e_{j1} *is* $F_1(e_{i1} - e_{j1})$*.*

(ii) The conditional probability that $s_{i2} + \eta s_{i1} > s_{j2} + \eta s_{j1}$ *given* e_{i2}*,* e_{j2} *and* Δs_{i1} *is* $F_2(\eta\Delta s_{i1} + e_{i2} - e_{j2})$*.*

1.4.1 Full revelation

In period 2, a player's information set consists of all combinations of period 1 efforts and error differences that are consistent with the own first-period effort e_{i1} and the observed relative performance Δs_{i1}.[22] We use the Perfect Bayesian Equilibrium (PBE) to deal with this imperfect information (Mas-Colell et al., 1995, p. 285). The task is simplified because there are no off-equilibrium events to consider, as f_1 is strictly positive on $\mathbb{R}$. Moreover, period 1 enters player i's payoffs only via Δs_{i1} and e_{i1}, so that the unobservable aspects of previous play (player j's effort choices) are irrelevant for the players' choices.

[21] The assumptions on the distribution of the error differences are guaranteed to hold if the assumptions hold for the distributions of the observation errors.

[22] This statement holds no matter whether the principal publicly announces the absolute performance of each agent, or just the difference.

A pure strategy σ_i of player i consists of a first-period choice e_{i1} and a function E_{i2} mapping information sets $(e_{i1}, \Delta s_{i1})$ to actions e_{i2}. If player i chose e_{i1}, observes Δs_{i1} and assumes that player j plays the pure strategy $\sigma_j = (e_{j1}, E_{j2})$, he will assign probability one to the event that $\Delta\varepsilon_{i1} = \Delta s_{i1} - \Delta e_{i1}$. We will always assume that beliefs are formed in this way, without specifying them explicitly.

1.4.1.1 Second-period efforts

Using Lemma 1.1 (ii), the expected second-period payoff of agent i, conditional on the relative first-period performance and second-period efforts, is

$$U_{i2}\left(e_{i2}, e_{j2}, \Delta s_{i1}\right) = F_2\left(\eta \Delta s_{i1} + \Delta e_{i2}\right) W_2 - K_2\left(e_{i2}\right). \tag{1.1}$$

Thus, the first period influences the second-period payoff via the first-period relative performance Δs_{i1}. The corresponding first-order condition is

$$f_2(\eta \Delta s_{i1} + \Delta e_{i2}) W_2 = K_2'\left(e_{i2}\right). \tag{1.2}$$

Though the game does not have any proper subgames because information sets in period 2 are not singletons, payoffs in period 2 are constant on information sets. We use this in the following definition.

Definition 1.2. The second-period effort game induced by Δs_{i1} is the game with players $i = 1, 2$, strategy spaces $X_i = \mathbb{R}^+$ and payoffs given by (1.1) for $(e_{i2}, e_{j2}) \in X_i \times X_j$.

We obtain the following result:

Lemma 1.2. *Suppose $\rho = 1$ (full revelation) and $W_2 > 0$.*

(i) In any equilibrium of the second-period effort game, efforts are symmetric and satisfy

$$e_{i2}^*\left(\Delta s_{i1}\right) \equiv e_{i2}^*\left(\Delta s_{i1}; \eta, W_2, 1\right) = \left(K_2'\right)^{-1}\left[f_2\left(\eta \Delta s_{i1}\right) W_2\right]. \tag{1.3}$$

(ii) If effort costs are sufficiently convex, (1.3) *defines the unique Nash equilibrium of the second-period effort game.*

Proof. See Appendix A1.1.1. □

Lemma 1.2 has some simple comparative statics implications.

Corollary 1.1. *Suppose $\rho = 1$, $\eta \neq 0$ and $W_2 > 0$. Then e_{i2}^* is decreasing in $|\Delta s_{i1}|$ and $|\eta|$, and increasing in W_2.*

Proof. See Appendix A1.1.2. □

The result on $|\Delta s_{i1}|$ implies that, if a *laggard* (an agent with $\Delta s_{i1} < 0$) increases or a *leader* (an agent with $\Delta s_{i1} > 0$) decreases own effort marginally in period 1, both players increase effort in period 2.[23] The other two results identify policy effects. In particular, increasing the absolute value of the first-period weight η reduces second-period efforts.

In the PBE, the symmetric equilibrium of the second-period effort game is played after each realization of Δs_{i1}. Thus, the expected second-period payoff, conditional on first-period performance, is

$$U_{i2}^{s}\left(\Delta s_{i1}\right) \equiv U_{i2}\left(e_{i2}^{*}\left(\Delta s_{i1}\right), e_{j2}^{*}\left(-\Delta s_{i1}\right), \Delta s_{i1}\right). \tag{1.4}$$

The expected second-period payoff, given first-period efforts, is

$$U_{i2}^{e}\left(e_{i1}, e_{j1}\right) \equiv E_{\Delta\varepsilon_{i1}} U_{i2}^{s}\left(\Delta e_{i1} + \Delta\varepsilon_{i1}\right). \tag{1.5}$$

1.4.1.2 First-period efforts

Using Lemma 1.1 (i), agent i's optimization problem in period 1 is

$$\max_{e_{i1} \geq 0} F_1\left(e_{i1} - e_{j1}\right) W_1 + U_{i2}^{e}\left(e_{i1}, e_{j1}\right) - K_1\left(e_{i1}\right).$$

The corresponding first-order conditions is

$$f_1\left(\Delta e_{i1}\right) W_1 + \frac{\partial U_{i2}^{e}}{\partial e_{i1}} = K_1'\left(e_{i1}\right). \tag{1.6}$$

The following definition is crucial for the intuition.

Definition 1.3. The *intensity of second-period competition* is given by

$$C\left(\eta\right) = 2\int_0^\infty f_2\left(\eta s\right) f_1\left(s\right) ds.$$

The logic of the definition is as follows. For each agent, $f_1(s)$ captures the density of the event that the relative first-period performance of this player is s when efforts are symmetric (as in equilibrium). Since both players choose identical equilibrium efforts in the second period, $f_2(\eta s) = f_2(-\eta s)$ captures the density of the event that a strike of luck of one agent in period 2 exactly compensates a strike of luck of the other agent of size s in period 1. Therefore, $C(\eta)$ measures the joint probability of the event that the second-period contest is a close run where a marginal effort increase of one agent will affect the outcome of the second-period contest and tip

[23] This result reflects the "well-known evaluation effect or lack-of-competition effect" (Ederer, 2010, p. 742)

the balance in his favor: When $C(\eta)$ is high, an agent who has been lucky in the first period cannot be too sure about his winning prospects in the second period, and will therefore continue to put in some effort.

$C(\eta)$ is a function of the weight η with several simple properties. First,

$$C'(\eta) = 2\int_0^{\infty} s f_2'(\eta s) f_1(s)\, ds < 0 \text{ for } \eta > 0. \tag{1.7}$$

An increase in the absolute value of the weight thus reduces the intensity of second-period competition. Moreover:

$$\begin{aligned} C(\eta) &> 0; \\ C(0) &= f_2(0); \qquad (1.8) \\ C'(0) &= 0; \qquad (1.9) \\ C(\eta) &= C(-\eta). \qquad (1.10) \end{aligned}$$

We sometimes invoke a regularity condition that simplifies the interpretation of our results:

Assumption 1.4. *For $\eta > 0$, $\eta C(\eta)$ is increasing in η.*

This conditions holds, for instance, in Example E1 below. The following condition rules out corner solutions in period 1:

$$f_1(0) W_1 + \eta W_2 C(\eta) > 0. \tag{1.11}$$

Note that (1.11) can only be binding for negative η.[24]

The following result uses (1.6) to derive equilibrium efforts:

Proposition 1.1. *Suppose $\rho = 1$ (full revelation).*

(i) In any symmetric interior PBE, first-period efforts must satisfy

$$e_1^*(\eta, W_1, W_2, 1) = (K_1')^{-1}\left[f_1(0) W_1 + \eta W_2 C(\eta)\right]. \tag{1.12}$$

(ii) Suppose the cost functions are sufficiently convex. If (1.11) *holds,* (1.3) *and* (1.12) *describe the unique symmetric PBE strategies. If* (1.11) *is violated,* $e_1^* = 0$ *and* (1.3) *describe the unique symmetric PBE strategies.*

Proof. See Appendix A1.1.3. □

We defer the discussion of the second-order conditions to the appendix; there we will show that they require sufficiently convex cost functions.[25]

[24] We will show below that the principal will never choose negative values for η.

[25] The relevant condition is (A1.8).

By Proposition 1.1, if Assumption 1.4 holds, then a higher positive weight of past effort always induces higher first-period effort. The term in brackets on the right-hand side of (1.12) is the marginal benefit from increasing e_{i1}. The effect on the expected first-period payoff is $f_1(0)\,W_1$; the effect on the expected second-period payoff is $\eta W_2 C(\eta)$, which is positive if $\eta > 0$. This term reflects the direct effect of higher first-period effort on second-period winning chances. The term does *not* capture strategic effects on the future efforts of the other player. Such effects are relevant in the game, but they cancel out in the symmetric equilibrium.[26]

We now characterize second-period efforts. Symmetry of the equilibrium in Proposition 1.1 implies $\Delta s_{i1} = \Delta\varepsilon_{i1}$. Using (1.3) and taking the expectation over $\Delta\varepsilon_{i1}$, we obtain:

Corollary 1.2. *The expected efforts in period* 2 *in the full-revelation PBE described in Proposition 1.1 are*

$$E\left(e_2^*\left(\eta, W_2, 1\right)\right) = 2\int_0^\infty \left(K_2'\right)^{-1}\left[f_2\left(\eta s\right)W_2\right] f_1\left(s\right) ds. \tag{1.13}$$

Proof. See Appendix A1.1.4. □

Together with Assumption 1.2, Corollary 1.2 implies that second-period efforts decrease in $|\eta|$. Thus, first-period efforts must increase at least locally in $|\eta|$ near the optimal η. Therefore, by (1.12), Assumption 1.4 must hold locally near the optimal η. Otherwise, by Proposition 1.1 the principal could increase efforts in both periods by reducing η, contradicting optimality of η.

1.4.2 No revelation

Under the no-revelation policy, agents simultaneously choose first- and second-period efforts according to

$$\begin{aligned} &\max_{e_{i1}\geq 0, e_{i2}\geq 0} F_1\left(e_{i1}-e_{j1}\right)W_1 && (1.14)\\ &+W_2\int_{-\infty}^{\infty} F_2\left(\eta\left(e_{i1}-e_{j1}+s\right)+e_{i2}-e_{j2}\right) f_1\left(s\right) ds - K_1\left(e_{i1}\right) - K_2\left(e_{i2}\right). \end{aligned}$$

[26] To see this, suppose $\eta > 0$; for $\eta < 0$, the argument is reversed. If, for any given first-period effort choice, a player knew he was ahead of the other player, he would have a strategic incentive to increase efforts to discourage player j from exerting effort in the future, whereas the converse would hold for a player who knows he is behind the opponent. Since the game is stochastic, players have to consider the expected strategic effects, which can be positive or negative, but cancel out when first-period efforts are identical.

The integral in (1.14) is the probability of winning the second-period prize, conditional on effort choices.[27] This leads to a simple characterization of the Nash equilibrium.

Proposition 1.2.

(i) Suppose $\rho = 0$ (no revelation). In any symmetric interior Nash equilibrium, efforts must satisfy:

$$e_1^*(\eta, W_1, W_2, 0) = (K_1')^{-1}[f_1(0)W_1 + \eta W_2 C(\eta)] > 0; \tag{1.15}$$

$$e_2^*(\eta, W_2, 0) = (K_2')^{-1}[W_2 C(\eta)] > 0. \tag{1.16}$$

(ii) If the cost functions are sufficiently convex and (1.11) *holds,* (1.15) *and* (1.16) *describe the unique symmetric Nash equilibrium of the game.*[28]

Proof. See Appendix A1.1.5. □

Both effort levels reflect standard cost-benefit considerations. The marginal benefit of first-period efforts depends on the increased winning probabilities in period 2 ($\eta C(\eta)$) and period 1 ($f_1(0)$).

By Proposition 1.1 and Proposition 1.2, first-period efforts in any symmetric equilibrium are non-stochastic and equal under both revelation policies; we thus write $e_1^*(\eta, W_1, W_2)$ for first-period equilibrium efforts.[29]

1.5 Optimal policy

We now characterize the optimal policy of the principal.[30] To this end, we fix the total budget as W, so that $W_2 = W - W_1$. Since we focus on symmetric equilibria and efforts within periods are perfect substitutes, we can write the principal's objective in terms of the efforts of only one agent. As first-period efforts are non-stochastic,

[27] This follows from Lemma 1.1 (ii).

[28] In Appendix A1.1.6 we identify the meaning of "sufficient convexity". We also show that the second-order conditions hold locally for arbitrary convex cost function.

[29] The result reflects the fact that the marginal effect of first-period effort on expected second-period payoffs is identical under both policies. Intuitively, a marginal increase of e_{i1} has positive effects on the second-period payoffs of player i if it suffices to tip the balance in the contest in period 2 in his favor. The probability that this happens, which is captured by $C(\eta)$ for both players, is independent of whether information on Δs_{i1} is revealed to players before they choose second-period efforts. In this argument, it is important to start from the respective equilibrium, with equal efforts in both periods.

[30] In the following discussion, we assume that, for given error distributions and effort cost functions, second-order conditions hold for all allowable choices of the policy variables. This is for instance true for the normal-quadratic example of Section 1.6.

the principal's objective functions for perfect and imperfect substitutes, respectively, are:

$$V^P(\eta, W_1, \rho) \equiv e_1^*(\eta, W_1, W - W_1) + E(e_2^*(\eta, W - W_1, \rho)); \tag{1.17}$$
$$V^I(\eta, W_1, \rho) \equiv e_1^*(\eta, W_1, W - W_1) \cdot E(e_2^*(\eta, W - W_1, \rho)). \tag{1.18}$$

1.5.1 Optimal revelation policy

According to (1.17) and (1.18), the principal chooses the revelation policy that induces higher expected second-period efforts, no matter whether efforts are perfect or imperfect substitutes. Using Jensen's inequality, we can compare the expected second-period efforts in the equilibria characterized by Proposition 1.1 and Proposition 1.2:[31]

Proposition 1.3. *$\forall\ \eta \in \mathbb{R}, W_1 < W$:*
(i) If $K_2''' \geq 0$, then $e_2^*(\eta, W - W_1, 0) \geq E(e_2^*(\eta, W - W_1, 1))$.
(ii) If $K_2''' \leq 0$, then $e_2^*(\eta, W - W_1, 0) \leq E(e_2^*(\eta, W - W_1, 1))$.

Proof. See Appendix A1.2. □

For quadratic costs, (i) and (ii) together imply that expected second-period efforts are equal under both revelation policies.[32] Proposition 1.3 applies to all values of η and W_1 and, in particular, to those that maximize $e_2^*(\eta, W - W_1, 0)$ or $E(e_2^*(\eta, W - W_1, 1))$. Thus, even if the principal has chosen the optimal parameters for a given revelation policy, switching to the other revelation policy is beneficial if the corresponding condition on K_2''' holds. Hence, we have proven:

Corollary 1.3. *The optimal revelation policy is the same for perfect and imperfect substitutes, with $\rho = 0$ if $K_2''' > 0$ and $\rho = 1$ if $K_2''' < 0$. For $K_2''' = 0$, expected payoffs are independent of the revelation policy.*

The result extends Aoyagi (2010) who shows that, for one prize ($W_1 = 0$) and equal weights ($\eta = 1$), the effort cost function completely determines the optimal revelation policy.[33] Our result shows that this statement holds for arbitrary W_1 and η.

[31]Intuitively, with revelation, the agents base their second-period decisions on the revealed asymmetry between players, whereas, without revelation, the expected asymmetry is decisive. Compare second-period decisions with and without revelation for given effort choices in the first period: For error realizations where the asymmetry is low (high) relative to expectations, efforts will be higher (lower) with revelation than without.

[32]Intuitively, the role of K_2''' is an immediate consequence of the fact that second-period efforts are the inverse of marginal costs for $\rho = 0$ and the expectation of the inverse of marginal costs for $\rho = 1$. Thus, concavity (convexity) of the inverse marginal costs is decisive for which regime yields higher efforts on expectation.

[33]Ederer (2010) also treats this case in his discussion of non-complementary abilities.

1.5.2 Optimal weight of past performance

The principal can give incentives for first-period efforts with W_1 or η. The next result shows that, no matter how high the first-period prize is, the principal should always assign a positive weight to past performance in the second-period contest. For perfect substitutes, we denote the optimal choice of η conditional on W_1 and ρ as $\eta^P(W_1,\rho)$ and the optimal choice of W_1 conditional on η as $W_1^P(\eta,\rho)$. For imperfect substitutes, we write $\eta^I(W_1,\rho)$ and $W_1^I(\eta,\rho)$.

Proposition 1.4. *$\eta^P(W_1,\rho)>0$ and $\eta^I(W_1,\rho)>0\ \forall\ W_1<W$ and $\rho=0,1$.*

Proof. See Appendix A1.3.1. □

This result holds because, for $\eta=0$, the marginal effect of η on first-period effort is positive and bounded away from zero (a first-order effect), whereas it is zero for second-period effort (a second-order effect). To understand the latter point, note that the adverse effect of increasing $\eta>0$ on second-period efforts arises because the second-period contest becomes more asymmetric, that is, less competitive ($C'(\eta)<0$). As $C'(0)=0$, this adverse effect vanishes as η approaches 0.

Proposition 1.4 states that performance evaluation should always have some memory: Firms should consider not only the recent performance of employees and suppliers, but also the performance in the distant past. Similarly, students should not only be judged with respect to their recent performance. The open question is: How large should the "shadow of the past" be? To answer this question for perfect substitutes, the next result characterizes the weight of past performance for quadratic costs (Assumption 1.3). In this case, revelation and no revelation imply the same behavior. Thus, we write $\eta^P(W_1)\equiv\eta^P(W_1,0)=\eta^P(W_1,1)$, and similarly for $W_1^P(\eta)$. Furthermore, we write $\left(\eta^P,W_1^P\right)=\arg\max_{\eta,W_1}V^P(\eta,W_1)$.

Proposition 1.5. *Suppose Assumption 1.3 holds. Then, $\forall\ W_1<W$, $\eta^P(W_1)$ satisfies*

$$\left|\frac{C'(\eta)}{C(\eta)}\right|=\frac{1}{\frac{k_1}{k_2}+\eta}. \tag{1.19}$$

Proof. See Appendix A1.3.2. □

(1.19) captures the trade-off between strengthening first-period incentives and weakening second-period competition. Changes in the error distributions that increase the sensitivity $\left|\frac{C'(\eta)}{C(\eta)}\right|$ of second-period competition to the first-period weight η for all η reduce the optimal η.[34] Furthermore, the higher first-period marginal effort costs are compared to second-period marginal effort costs, the lower is the optimal

[34] We illustrate this in Figure 1.2 below.

η. Note that (1.19) and thus the optimal η is independent of the first-period prize W_1.[35]

1.5.3 Optimal first-period prize

We now supply several results on the optimal prize structure. We also use these results to obtain further insights on the optimal weights.

1.5.3.1 Perfect substitutes

For perfect substitutes, we confine ourselves to quadratic costs (Assumption 1.3) in the main text. The results are special cases of more general, but less transparent results that we state and prove in Appendix A1.4 (Proposition A1.1 and Proposition A1.2).

For quadratic costs, it is optimal to give only one positive prize. Depending on the observation error distributions, the prize should be based only on first-period performance ($W_1^P = W$) or on second-period performance as well ($W_1^P = 0$).

Corollary 1.4. *Suppose Assumption 1.3 holds.*

(i) If $f_1(0) < \left(\frac{k_1}{k_2} + \eta\right) C(\eta)$, then the optimal first-period prize conditional on η is $W_1^P(\eta) = 0$. If $\exists\, \eta \in \mathbb{R}$ s.t. $f_1(0) < \left(\frac{k_1}{k_2} + \eta\right) C(\eta)$, then the unconditonally optimal first-period prize is $W_1^P = 0$.

(ii) If $f_1(0) > \left(\frac{k_1}{k_2} + \eta\right) C(\eta)$, then the optimal first-period prize conditional on η is $W_1^P(\eta) = W$. If $f_1(0) > \left(\frac{k_1}{k_2} + \eta\right) C(\eta)\ \forall\ \eta \in \mathbb{R}$, then the unconditonally optimal first-period prize is $W_1^P = W$.

Proof. See Appendix A1.4.3. □

The intuition is straightforward. By (i), when second-period competition (as captured by $C(\eta)$) is intense enough relative to the precision of the first-period measurement (as captured by $f_1(0)$), then second-period efforts should be positive, which requires a second-period prize.[36] Otherwise (case (ii)), there should be no second-period prize. However, we will show in Section 1.6 that $W_1^P = 0$ always holds in a normal-quadratic example. Note that the condition under which there is no first-period prize is easier to satisfy when first-period marginal effort costs are high compared to second-period marginal effort costs.

---

[35] This is due to the fact that W_1 enters $\frac{\partial V^P(\eta, W_1)}{\partial \eta}$ linearly. The relevant expression is (A1.18). Since $\frac{\partial^2 V^P(\eta, W_1)}{\partial \eta \partial W_1} < 0$, the increase in payoff by setting η optimally depends positively on $W - W_1$, the prize paid in the second period.

[36] Note that $f_1(0)$ is a purely local measure of precision, capturing the probability that identical efforts translate into identical performance measures.

Beyond quadratic effort costs In Appendix A1.4, we provide results on the optimal first-period prizes and weights for general cost functions. These results imply Corollary 1.4 for $K_t''' = 0$ as a special case. When $K_t''' \neq 0$, the effort choices with and without information revelation are no longer identical, so that the optimal policies do not coincide. Proposition A1.1 in the Appendix characterizes the optimal prize structure with information revelation for $K_t''' \leq 0$, in which case information revelation is superior to no revelation by Proposition 1.3. Conversely, Proposition A1.2 in the Appendix characterizes the optimal prize structure without information revelation for $K_t''' \geq 0$, where no information revelation is superior to revelation by Proposition 1.3. The interpretation of the general propositions is similar as for quadratic costs: If the first-period contest is too noisy, it is optimal not to give a first-period prize.

1.5.3.2 Imperfect substitutes

For imperfect substitutes, we also obtain a general condition under which the optimal first-period prize for a given past weight is zero with performance revelation. The result applies if $K_t''' \leq 0$, so that revelation is optimal by Corollary 1.3.

Proposition 1.6. *Suppose $K_t''' \leq 0$ for $t = 1, 2$. For all $\eta > 0$, $W_1^I(\eta, 1) = 0$ if $f_1(0) < \eta C(\eta)$.*

Proof. See Appendix A1.5.1. □

Thus, as with perfect substitutes, this (sufficient) condition is easier to satisfy if the first-period signal is not very precise ($f_1(0)$ is low) and second-period competition $C(\eta)$ is intense. To obtain stronger results, we now specialize to quadratic effort costs. As behavior is the same with and without revelation, we write $\eta^I(W_1) \equiv \eta^I(W_1, 0) = \eta^I(W_1, 1)$ and similarly for $W_1^I(\eta)$. Furthermore, we write $\left(\eta^I, W_1^I\right) = \arg\max_{\eta, W_1} V^I(\eta, W_1)$. We obtain:

Proposition 1.7. *If Assumption 1.3 holds, the optimal first-period prize conditional on η is $W_1^I(\eta) \leq \frac{W}{2}$ $\forall\ \eta$, so that the optimal unconditional first-period prize is $W_1^I \leq \frac{W}{2}$.*

Proof. See Appendix A1.5.2. □

There is no counterpart of this result for perfect substitutes, where it can, in principle, be optimal to refrain from inducing second-period effort altogether. For imperfect substitutes, principals aim at a balanced effort distribution. Therefore, they need to make sure not to give excessive first-period prizes, because they are already providing indirect incentives for first-period effort through the weight η.

The following result specifies the optimal solution further:

Proposition 1.8. *Suppose Assumption 1.3 holds.*

(i) $W_1^I(\eta) > 0$ *if and only if* $f_1(0) > 2\eta C(\eta)$. *In this case*

$$W_1^I(\eta) = W \frac{f_1(0) - 2\eta C(\eta)}{2f_1(0) - 2\eta C(\eta)} > 0.$$

(ii) The optimal $\left(W_1^I, \eta^I\right)$ *satisfies one of the following necessary properties:*

(a) $W_1^I = 0$ *and* $\left|\frac{C'(\eta^I)}{C(\eta^I)}\right| = \frac{1}{2\eta^I}$.

(b) $W_1^I = W \frac{f_1(0) - 2\eta^I C\left(\eta^I\right)}{2f_1(0) - 2\eta^I C(\eta^I)} > 0$ *and* $\left|\frac{C'(\eta^I)}{C(\eta^I)}\right| = \frac{C\left(\eta^I\right)}{f_1(0)}$.

Proof. See Appendix A1.5.3. □

Result (i) describes the optimal prize structure conditional on η. As with perfect substitutes, the optimal first-period prize is positive if first-period precision (captured by $f_1(0)$) is high and second-period competition $C(\eta)$ is low. Moreover, the result sharpens Proposition 1.6 by showing that, at least for quadratic costs, $f_1(0) < \eta C(\eta)$ is not necessary to guarantee that the conditionally optimal prize structure satisfies $W_1^I(\eta) = 0$. Finally, the result shows that, when Assumption 1.3 and Assumption 1.4 hold, first-period prizes and weights are substitutes: The optimal first-period prize is lower the higher the first-period weight η is.

Result (ii) describes the unconditionally optimal solution $\left(W_1^I, \eta_1^I\right)$ for quadratic costs. There are two possibilities, both depicted in Figure 1.1. According to (a), the first-period prize may be zero, in which case the optimal first-period weight is described by a simple condition that depends exclusively on $C(\eta)$ (see point A on the horizontal axis in Figure 1.1). As with perfect substitutes, the weight is lower if the adverse effect of η on future competition is higher. By (b), the first-period prize may be positive, in which case the optimal first-period weight is determined by a condition that depends on error distributions not only via $C(\eta)$, but also via $f_1(0)$ directly, as captured by $W_1^I(\eta)$ (see point B on the diagonal line in Figure 1.1).[37] The error distributions determine which of the two cases in Proposition 1.8 applies. For instance, with normal error distributions, the first-period prize is zero (see Corollary 1.6 below).

Note that in contrast to the perfect substitutes case (Proposition 1.5 and Corollary 1.4), the optimal weights and prizes are independent of the relation between first- and second-period effort costs and entirely determined by the properties of the observation error distributions.

[37]Note that $W_1^I(\eta)$ is typically not linear.

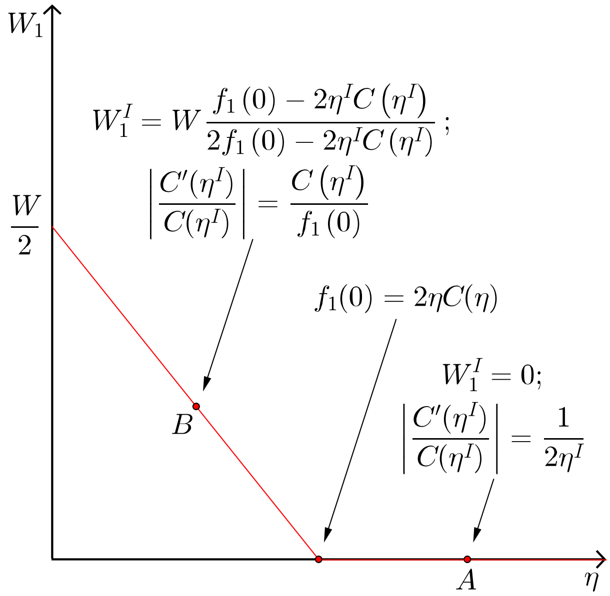


Figure 1.1: Necessary conditions for imperfect substitutes

1.5.4 Restrictions on allowable policies

In some circumstances, principals may not be free to choose arbitrary incentive systems. For instance, there may be a limit on the extent to which they may consider past performance in current evaluations. Then first-period prizes may act as substitutes for performance weights: For instance, with perfect substitutes, Corollary 1.4 shows that it is weakly easier to satisfy the condition for $W_1^P(\eta) = W$ and correspondingly weakly more difficult to satisfy the condition for $W_1^P(\eta) = 0$ when η is bounded above. Conversely, the prize structure may be restricted. For instance, a principal may have to spread the prize sum evenly. According to Proposition 1.7, $W_1 \leq \frac{W}{2}$ must hold for the optimal first-period prize with imperfect substitutes. In cases where an unconstrained principal would set $W_1 < \frac{W}{2}$, a principal who has to set $W_1 = \frac{W}{2}$ is giving excessive first-period incentives relative to the unconstrained case. To make up for this, she has to adjust the weight of first-period performance downwards.

1.6 A normal-quadratic example

To obtain sharper results, we introduce a simple example.

Example E1. The cost function is $K_t(e_{it}) = \frac{k}{2}e_{it}^2$ for $t = 1, 2$. The error difference $\Delta\varepsilon_{it}$ is normally distributed with variance σ_t^2.[38]

Example E1 satisfies Assumption 1.1 and Assumption 1.2.[39]

Corollary 1.5. *In E1, a PBE exists. The equilibrium efforts under revelation and no revelation are*

$$e_1^*(\eta, W_1, W_2) = \frac{1}{k\sqrt{2\pi}}\left(\frac{W_1}{\sigma_1} + \frac{\eta W_2}{\sqrt{\sigma_2^2 + \sigma_1^2\eta^2}}\right); \tag{1.20}$$

$$e_2^*(\eta, W_2, 0) = E\left(e_2^*(\eta, W_2, 1)\right) = \frac{W_2}{k\sqrt{2\pi}\sqrt{\sigma_2^2 + \sigma_1^2\eta^2}}. \tag{1.21}$$

Proof. See Appendix A1.6.1. □

Comparative statics for second-period efforts are straightforward. Lower marginal effort costs, higher second-period prize, lower first-period weight and higher first- and second-period precision induce higher second-period efforts. Analogous results hold for period one. First-period efforts also increase if the second-period precision increases, given $\eta > 0$: The parameter change makes first-period effort more worthwhile, because the positive effect on winning the second-period prize increases. Finally, a redistribution of the prize sum from period 2 to period 1 increases first-period efforts, because the positive effect of an increase in the first-period prize is always stronger than the negative effect of an identical decrease in the second-period prize.

Figure 1.2 illustrates Proposition 1.5 for Example E1. It shows how the optimal weight η^P depends on the sensitivity $\left|\frac{C'(\eta)}{C(\eta)}\right|$ of second-period competition to η, which is low if the second-period performance measurement is relatively imprecise compared to the first-period measurement, implying a higher optimal weight of first-period performance.

Corollary 1.6 characterizes the optimal policy. The results endogenize the assumption that $W_1 = 0$ and $\eta = 1$ in Aoyagi (2010) for identically normally distributed error distributions ($\sigma_1 = \sigma_2$).[40]

Corollary 1.6. *In E1,*

(i) $\eta^P(W_1) = \frac{\sigma_2^2}{\sigma_1^2}\ \forall\ W_1 < W$. *Furthermore,* $\eta^P = \frac{\sigma_2^2}{\sigma_1^2}$ *and* $W_1^P = 0$.

(ii) necessary conditions for the optimum are $\eta^I = \frac{\sigma_2}{\sigma_1}$ *and* $W_1^I = 0$.

[38] A normally distributed error difference follows, for example, from normally distributed observation errors.

[39] In the appendix, we also derive the second-order conditons ((A1.37) and (A1.39)).

[40] Similarly, we provide a justification for the model of Ederer (2010) with non-complementary abilities in which $W_1 = 0$ and $\eta = 0$.

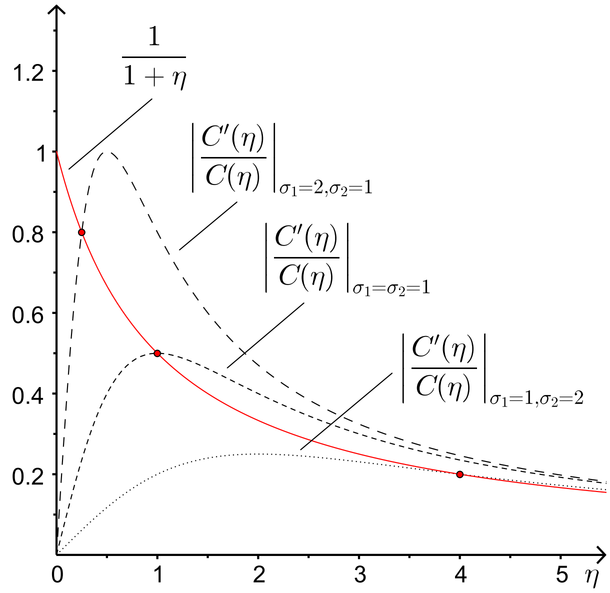


Figure 1.2: Necessary conditions for η with perfect substitutes

Proof. See Appendix A1.6.2. □

(i) shows that it is optimal to give only a second-period prize with perfect substitutes. Incentives for first-period efforts come exclusively from the weight of past performance, which is the ratio of the error variances in the two periods. (ii) yields a similar result for imperfect substitutes, with variances replaced by standard deviations. Note that $\eta^I > \eta^P$ if and only if $\frac{\sigma_2}{\sigma_1} < 1$: Greater precision of the second-period performance measure leads to higher second-period efforts compared to the first-period efforts. With imperfect substitutes, where an even effort flow is desired, a greater weight of the first period is used to mitigate the asymmetry.

The example demonstrates the importance of the right incentive system. To see this, suppose that initially the principal chooses $(\eta, W_1) = \left(0, \frac{W}{2}\right)$, so that there are two independent and identical tournaments. Next, suppose that the principal introduces the optimal policy in two steps: First, she maintains the equal division of the prize sum across periods, but chooses the optimal weight $\eta^P\left(\frac{W}{2}\right)$, so that $(\eta, W_1) = (\frac{\sigma_2^2}{\sigma_1^2}, \frac{W}{2})$. Finally, she chooses prizes and weights optimally, so that $(\eta, W_1) = (\frac{\sigma_2^2}{\sigma_1^2}, 0)$. Simple calculations (available on request) show that if the principal sets only η optimally, the relative increase of her payoff, compared to the initial situation, is

$$E_\eta^P \equiv \frac{V^P\left(\frac{\sigma_2^2}{\sigma_1^2}, \frac{W}{2}\right) - V^P\left(0, \frac{W}{2}\right)}{V^P\left(0, \frac{W}{2}\right)} = \frac{\sqrt{\sigma_1^2+\sigma_2^2} - \sigma_1}{(\sigma_1+\sigma_2)}.$$

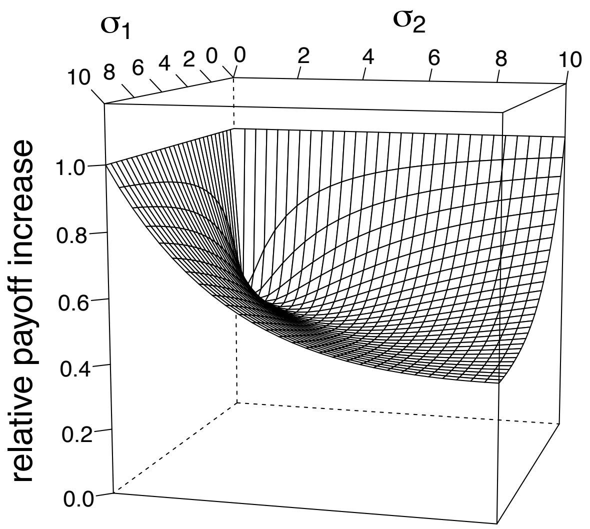


Figure 1.3: Relative payoff increase when setting W_1 and η optimally

By optimally adjusting the prize structure as well, she achieves an additional relative payoff increase of

$$E^P_{W_1} \equiv \frac{V^P\left(\frac{\sigma_2^2}{\sigma_1^2}, 0\right) - V^P\left(\frac{\sigma_2^2}{\sigma_1^2}, \frac{W}{2}\right)}{V^P\left(\frac{\sigma_2^2}{\sigma_1^2}, \frac{W}{2}\right)} = \frac{\sqrt{\sigma_1^2+\sigma_2^2}-\sigma_2}{\sqrt{\sigma_1^2+\sigma_2^2}+\sigma_2}.$$

The relative importance of these two effects depends on the precision of the performance measures. If second-period performance measurement is very precise ($\sigma_2 \approx 0$), whereas the first-period measure is not, then $E^P_\eta \approx 0$; If first-period performance measurement is very precise ($\sigma_1 \approx 0$), whereas the second-period measure is not, then $E^P_{W_1} \approx 0$. Thus, getting the performance weight right (rather than choosing $\eta = 0$) is only important when second-period performance measurement is imprecise; getting the second-period prize right (rather than splitting the price equally) only matters when first-period performance measurement is imprecise.

Compared to the initial situation, the total relative payoff increase from setting both W_1 and η optimally is

$$E^P_{\eta,W_1} \equiv \frac{V^P\left(\frac{\sigma_2^2}{\sigma_1^2}, 0\right) - V^P\left(0, \frac{W}{2}\right)}{V^P\left(0, \frac{W}{2}\right)} = \frac{2\sqrt{\sigma_1^2+\sigma_2^2}-(\sigma_1+\sigma_2)}{(\sigma_1+\sigma_2)}.$$

Figure 1.3 shows how the total relative payoff increase from choosing the optimal incentive system depends on the variances of the error distributions.

E^P_{η,W_1} attains its minimum for $\sigma_1 = \sigma_2$ at $\sqrt{2} - 1 \approx 41\%$. Thus, the percentage payoff increase from implementing the optimal policy is lowest if both performance measurements are equally precise. Even in this case, however, the benefits are substantial: The principal can achieve 41% higher payoff with a budget-neutral policy adjustment. Figure 1.3 further shows that if one of the performance measures is very precise ($\sigma_t \approx 0$), then $E^P_{\eta,W_1} \approx 1$. Hence, the more precise one of the performance measures, the more the principal can benefit from implementing the optimal policy.

1.7 Concluding remarks

This study analyzes intertemporal effort provision in two-stage tournaments. A principal with a fixed budget for prizes faces two risk-neutral agents. She observes noisy signals of effort in both periods. She aims at maximizing either total efforts (perfect substitutes) or the product of first- and second-period efforts (imperfect substitutes). She decides (i) how to spread prize money across the two periods, (ii) how to weigh performance in the two periods when awarding the second-period prize, and (iii) whether to reveal performance after the first period.

We obtain several new insights. First, design matters. The potential losses from suboptimal incentive systems are substantial. Second, several interesting results of existing research on revelation policy and performance weights are much more general than previously known, extending in particular to the important case that efforts in different periods are not perfect substitutes. Third, we provide new results on the determinants of optimal incentives. We show that the weight of past performance should depend negatively on the extent to which a higher weight of the past reduces competition. We also show how the spread of prizes across periods and the choice of weights depends on the relative precision of performance measures in the two periods. Finally, we show that, under quite general conditions, there should be no first-period prize.

Acknowledgements

The authors thank Andreas Hefti, Paul Heidhues, Stefan Jönsson, Igor Letina, John Morgan, Nick Netzer, Georg Nöldeke, Ron Siegel and seminar audiences in Aarhus (Spring Meeting of Young Economists), Düsseldorf (Verein für Socialpolitik), Fresno (Conference on Tournaments, Contests and Relative Performance Evaluation), Lisbon (UECE Meetings), Lucerne (Zurich Workshop on Economics) and Zurich for helpful discussions. Shuo Liu provided excellent research assistance. Klein gratefully acknowledges financial support of the Swiss National Science Foundation (grant numbers 131854 and 151688).

Appendix

A1.1 Behavior of the agents

The proofs in this section generalize Aoyagi (2010) and Ederer (2010) (for non-complementary abilities) who assume $W_1 = 0$ and $\eta = 1$.

A1.1.1 Proof of Lemma 1.2

Proof of part (i). Equilibrium efforts must be positive because $f_2 > 0$ by Assumption 1.2 and $K_2'(0) = 0$ by Assumption 1.1. Since f_2 is symmetric by Assumption 1.2 and

$$-(\eta \Delta s_{11} + \Delta e_{12}) = \eta \Delta s_{21} + \Delta e_{22},$$

the left-hand side of the first-order condition (1.2) is equal for both agents. Hence, $e_{i2}^*(\Delta s_{i1}) = e_{j2}^*(-\Delta s_{i1})$, so that the second-period efforts are the same for both agents. Thus, (1.2) becomes $f_2(\eta \Delta s_{i1}) W_2 = K_2'(e_{i2})$. As $K_2'' > 0$ by Assumption 1.1, K_2' is strictly increasing and thus invertible. Thus (1.3) must hold in any equilibrium.

Proof of part (ii). The following inequality guarantees that the second-period payoffs (1.1) of player i are strictly concave in e_{i2}:

$$f_2'(\eta \Delta s_{i1} + \Delta e_{i2}) W_2 < K_2''(e_{i2}) \ \forall \ \Delta s_{i1} \in \mathbb{R}; \forall \ e_{i2}, e_{j2} \in \mathbb{R}^+. \quad \text{(A1.1)}$$

(A1.1) requires K_2 to be sufficiently convex.[41] If this condition holds globally, the first-order conditions (1.2) characterize a Nash equilibrium. Moreover, the equilibrium is unique, as (1.3) must necessarily hold in any equilibrium by part (i) of the lemma.

A1.1.2 Proof of Corollary 1.1

The inverse function theorem yields

$$\left[(K_2')^{-1}\right]'(f_2(\eta \Delta s_{i1}) W_2) = \frac{1}{K_2''\left((K_2')^{-1}(f_2(\eta \Delta s_{i1}) W_2)\right)}.$$

[41]By Assumption 1.2, $f_2'(\eta \Delta s_{i1} + \Delta e_{i2}) < 0$ if $\eta \Delta s_{i1} + \Delta e_{i2} > 0$, so that (A1.1) always holds in this case. For the case that $\eta \Delta s_{i1} + \Delta e_{i2} < 0$, suppose f_2' is bounded above. Then (A1.1) holds globally if K_2'' has a sufficiently high lower bound.

Thus (1.3) implies

$$\frac{\partial e_{i2}}{\partial \Delta s_{i1}} = \frac{\eta f_2'(\eta \Delta s_{i1}) W_2}{K_2''\left((K_2')^{-1}(f_2(\eta \Delta s_{i1}) W_2)\right)};$$
$$\frac{\partial e_{i2}}{\partial \eta} = \frac{\Delta s_{i1} f_2'(\eta \Delta s_{i1}) W_2}{K_2''\left((K_2')^{-1}(f_2(\eta \Delta s_{i1}) W_2)\right)};$$
$$\frac{\partial e_{i2}}{\partial W_2} = \frac{f_2(\eta \Delta s_{i1})}{K_2''\left((K_2')^{-1}(f_2(\eta \Delta s_{i1}) W_2)\right)}. \tag{A1.2}$$

By Assumption 1.1, $K_2'' > 0$. By Assumption 1.2, if $\Delta s_{i1} < (>)\ 0 \wedge \eta \neq 0 \wedge W_2 > 0$, then $\eta f_2'(\eta \Delta s_{i1}) > (<)\ 0$ and thus $\frac{\partial e_{i2}}{\partial \Delta s_{i1}} > (<)\ 0$. This implies that e_{i2} is decreasing in $|\Delta s_{i1}|$. As $\Delta s_{i1} = \Delta e_{i1} + \Delta \varepsilon_{i1}$ we obtain the results for e_{i1} and e_{j1}. Similar arguments show that $\frac{\partial e_{i2}}{\partial \eta} > (<)\ 0$ for $\eta < (>)\ 0$ and thus e_{i2} decreasing in $|\eta|$. Since $f_2 > 0$ by Assumption 1.2, we have $\frac{\partial e_{i2}}{\partial W_2} > 0$.

A1.1.3 Proof of Proposition 1.1

Proof of part (i). We first derive expressions for $\frac{\partial U_{i2}^e}{\partial e_{i1}}$ for symmetric first-period efforts. This allows us to state the FOC.

Lemma A1.1.

$$\left.\frac{\partial U_{i2}^e}{\partial e_{i1}}\right|_{e_{i1}=e_{j1}} = \eta W_2 C(\eta) \tag{A1.3}$$

Proof. Applying the envelope theorem to (1.4), we obtain

$$\frac{dU_{i2}^s(\Delta s_{i1})}{d\Delta s_{i1}} = \frac{\partial U_{i2}}{\partial e_{j2}} \frac{\partial e_{j2}^*(-\Delta s_{i1})}{\partial \Delta s_{i1}} + \frac{\partial U_{i2}}{\partial \Delta s_{i1}}. \tag{A1.4}$$

Using (A1.2) and the symmetry of the density (Assumption 1.2),

$$\frac{\partial e_{j2}^*(-\Delta s_{i1})}{\partial \Delta s_{i1}} = \frac{\partial e_{i2}(\Delta s_{i1})}{\partial \Delta s_{i1}} = \frac{\eta f_2'(\eta \Delta s_{i1}) W_2}{K_2''\left((K_2')^{-1}(f_2(\eta \Delta s_{i1}) W_2)\right)}.$$

(1.1) implies

$$\frac{\partial U_{i2}}{\partial e_{j2}} = -f_2(\eta \Delta s_{i1} + \Delta e_{i2}) W_2;$$
$$\frac{\partial U_{i2}}{\partial \Delta s_{i1}} = \eta f_2(\eta \Delta s_{i1} + \Delta e_{i2}) W_2.$$

Using these equations in (A1.4) and inserting $\Delta e_{i2} = 0$, we obtain

$$\frac{dU_{i2}^s}{d\Delta s_{i1}} = -\frac{\eta f_2(\eta \Delta s_{i1}) f_2'(\eta \Delta s_{i1}) W_2^2}{K_2''\left((K_2')^{-1}(f_2(\eta \Delta s_{i1}) W_2)\right)} + \eta f_2(\eta \Delta s_{i1}) W_2.$$

Using this in (1.5), we obtain

$$\begin{aligned}&\frac{\partial U_{i2}^e}{\partial e_{i1}} = \\ &\int_{-\infty}^{\infty}\left[-\frac{\eta f_2(\eta(\Delta e_{i1}+s)) f_2'(\eta(\Delta e_{i1}+s)) W_2^2}{K_2''\left((K_2')^{-1}(f_2(\eta(\Delta e_{i1}+s)) W_2)\right)} + \eta f_2(\eta(\Delta e_{i1}+s)) W_2\right] f_1(s)\, ds \\ &= \eta W_2 \int_{-\infty}^{\infty} f_2(\eta(\Delta e_{i1}+s)) f_1(s)\, ds \\ &-\eta W_2^2 \int_{-\infty}^{\infty} \frac{f_2(\eta(\Delta e_{i1}+s)) f_2'(\eta(\Delta e_{i1}+s))}{K_2''\left((K_2')^{-1}(f_2(\eta(\Delta e_{i1}+s)) W_2)\right)} f_1(s)\, ds.\end{aligned}$$

Let

$$\begin{aligned}A &:= \int_{-\infty}^{\infty} f_2(\eta(\Delta e_{i1}+s)) f_1(s)\, ds; \\ B &:= \int_{-\infty}^{\infty} \frac{f_2(\eta(\Delta e_{i1}+s)) f_2'(\eta(\Delta e_{i1}+s))}{K_2''\left((K_2')^{-1}(f_2(\eta(\Delta e_{i1}+s)) W_2)\right)} f_1(s)\, ds.\end{aligned}$$

With this notation,

$$\frac{\partial U_{i2}^e}{\partial e_{i1}} = \eta W_2 A - \eta W_2^2 B. \tag{A1.5}$$

Substituting $s = t - \Delta e_{i1}$ and $ds = dt$ in A and decomposing the integral gives

$$A = \int_{-\infty}^{0} f_2(\eta t) f_1(t - \Delta e_{i1})\, dt + \int_{0}^{\infty} f_2(\eta t) f_1(t - \Delta e_{i1})\, dt.$$

Let $u = -t$. Symmetry of f_1 and f_2 by Assumption 1.2 implies $f_2(\eta t) = f_2(\eta u)$ and $f_1(t - \Delta e_{i1}) = f_1(u + \Delta e_{i1})$. Hence,

$$\int_{-\infty}^{0} f_2(\eta t) f_1(t - \Delta e_{i1})\, dt = \int_{0}^{\infty} f_2(\eta u) f_1(u + \Delta e_{i1})\, du.$$

Thus,

$$\begin{aligned}A &= \int_{0}^{\infty} f_2(\eta u) f_1(u + \Delta e_{i1})\, du + \int_{0}^{\infty} f_2(\eta t) f_1(t - \Delta e_{i1})\, dt \\ &= \int_{0}^{\infty} f_2(\eta t)\left[f_1(t + \Delta e_{i1}) + f_1(t - \Delta e_{i1})\right] dt.\end{aligned}$$

Substituting $s = t - \Delta e_{i1}$ and $ds = dt$ in B and decomposing the integral, we obtain

$$B = \int_{-\infty}^{0} \frac{f_2(\eta t) f_2'(\eta t) f_1(t - \Delta e_{i1})}{K_2''\left((K_2')^{-1}(f_2(\eta t) W_2)\right)} dt + \int_{0}^{\infty} \frac{f_2(\eta t) f_2'(\eta t) f_1(t - \Delta e_{i1})}{K_2''\left((K_2')^{-1}(f_2(\eta t) W_2)\right)} dt.$$

Again using $u = -t$ and appealing to symmetry, $f_2(\eta t) = f_2(\eta u)$, $f_2'(\eta t) = -f_2'(\eta u)$ and $f_1(t - \Delta e_{i1}) = f_1(u + \Delta e_{i1})$. Thus

$$\int_{-\infty}^{0} \frac{f_2(\eta t) f_2'(\eta t) f_1(t - \Delta e_{i1})}{K_2''\left((K_2')^{-1}(f_2(\eta t) W_2)\right)} dt = \int_{0}^{\infty} \frac{f_2(\eta u)(-f_2'(\eta u)) f_1(u + \Delta e_{i1})}{K_2''\left((K_2')^{-1}(f_2(\eta u) W_2)\right)} du.$$

Hence,

$$\begin{aligned} B &= \int_{0}^{\infty} \frac{-f_2(\eta u) f_2'(\eta u) f_1(u + \Delta e_{i1})}{K_2''\left((K_2')^{-1}(f_2(\eta u) W_2)\right)} du + \int_{0}^{\infty} \frac{f_2(\eta t) f_2'(\eta t) f_1(t - \Delta e_{i1})}{K_2''\left((K_2')^{-1}(f_2(\eta t) W_2)\right)} dt \\ &= \int_{0}^{\infty} \frac{f_2(\eta t) f_2'(\eta t)\left[-f_1(t + \Delta e_{i1}) + f_1(t - \Delta e_{i1})\right]}{K_2''\left((K_2')^{-1}(f_2(\eta t) W_2)\right)} dt. \end{aligned}$$

Substituting the expressions for A and B into (A1.5) and using $s = t$, we obtain

$$\begin{aligned} \frac{\partial U_{i2}^e}{\partial e_{i1}} &= \eta W_2 \int_{0}^{\infty} f_2(\eta s)\left[f_1(s + \Delta e_{i1}) + f_1(s - \Delta e_{i1})\right] ds \\ &\quad + \eta W_2^2 \int_{0}^{\infty} \frac{f_2(\eta s) f_2'(\eta s)\left[f_1(s + \Delta e_{i1}) - f_1(s - \Delta e_{i1})\right]}{K_2''\left((K_2')^{-1}(f_2(\eta s) W_2)\right)} ds. \end{aligned} \tag{A1.6}$$

With $\Delta e_{i1} = 0$, we obtain (A1.3). □

Together, (1.6) and Lemma A1.1 imply

$$f_1(0) W_1 + \eta W_2 C(\eta) = K_1'(e_{i1}).$$

By Assumption 1.1, K_1' is invertible. We thus obtain (1.12) as a necessary condition for any symmetric interior PBE.

Proof of part (ii). We know from Lemma 1.2 (ii) that (1.2) implies sequential rationality in the second period. Moreover, from the discussion at the beginning of Section 1.4.1, beliefs are consistent.

As $K_1'(0) = 0$ by Assumption 1.1, efforts must be positive in any symmetric equilibrium if (1.11) holds. Thus, by part (i), (1.12) is a necessary condition for an equilibrium. The second-order condition for player i is

$$f_1'(\Delta e_{i1}) W_1 + \frac{\partial^2 U_{i2}^e}{(\partial e_{i1})^2} < K_1''(e_{i1}) \ \ \forall \ e_{i1}, e_{j1} \in \mathbb{R}^+. \tag{A1.7}$$

Inserting (A1.6) in (A1.7) gives

$$\begin{aligned} f_1'(\Delta e_{i1}) W_1 + \eta W_2 \int_0^\infty f_2(\eta s)\left[f_1'(s+\Delta e_{i1}) - f_1'(s-\Delta e_{i1})\right] ds \\ + \eta W_2^2 \int_0^\infty \frac{f_2(\eta s) f_2'(\eta s)\left[f_1'(s+\Delta e_{i1}) + f_1'(s-\Delta e_{i1})\right]}{K_2''\left((K_2')^{-1}(f_2(\eta s) W_2)\right)} ds < K_1''(e_{i1}). \end{aligned} \tag{A1.8}$$

The left-hand side of this inequality is decreasing in K_2'', while the right-hand side is increasing in K_1''. For given policy parameters and distributions, (A1.7) therefore holds as long as $\min\{K_1''(0), K_2''(0)\}$, which is a lower bound for $K_1''(e_{i1})$ and $K_2''\left((K_2')^{-1}(f_2(\eta s) W_2)\right)$, is sufficiently large. In this case, the second-order condition can be guaranteed to hold whenever the slopes of f_1 and f_2 are bounded.

If these conditions hold globally, (1.12) thus describes an equilibrium, which is the unique symmetric equilibrium.

A1.1.4 Proof of Corollary 1.2

Symmetry of the equilibrium implies $\Delta s_{i1} = \Delta\varepsilon_{i1}$. Hence, (1.3) implies

$$e_{i2}^*(\Delta s_{i1}, \eta, W_2, 1) = (K_2')^{-1}(f_2(\eta\Delta\varepsilon_{i1}) W_2).$$

Taking the expectation over $\Delta\varepsilon_{i1}$, we obtain

$$E_{\Delta\varepsilon_{i1}}(e_{i2}^*(\Delta s_{i1}, \eta, W_2, 1)) = \int_{-\infty}^{\infty} (K_2')^{-1}(f_2(\eta s) W_2) f_1(s)\, ds.$$

From the symmetry of the density by Assumption 1.2, we get (1.13).

A1.1.5 Proof of Proposition 1.2

Proof of part (i). From (1.14), the first-order conditions are

$$\begin{aligned} f_1(\Delta e_{i1}) W_1 + \eta W_2 \int_{-\infty}^{\infty} f_2(\eta(\Delta e_{i1}+s) + \Delta e_{i2}) f_1(s)\, ds &= K_1'(e_{i1}); \\ W_2 \int_{-\infty}^{\infty} f_2(\eta(\Delta e_{i1}+s) + \Delta e_{i2}) f_1(s)\, ds &= K_2'(e_{i2}). \end{aligned}$$

For the symmetric case $\Delta e_{i1} = \Delta e_{i2} = 0$, this simplifies to

$$\begin{aligned} f_1(0) W_1 + \eta W_2 C(\eta) &= K_1'(e_{i1}); \\ W_2 C(\eta) &= K_2'(e_{i2}). \end{aligned}$$

Inverting K_1' and K_2' yields (1.15) and (1.16).

Proof of part (ii). If (1.11) holds, first-period equilibrium efforts are positive because $K_1'(0) = 0$ by Assumption 1.1. Equilibrium efforts in the second period are positive because $W_2 C(\eta) > 0$ by Assumption 1.2. By part (i), (1.15) and (1.16) are necessary equilibrium conditions.

Consider the following second-order conditions:[42]

$$f_1'(\Delta e_{i1}) W_1 + \eta^2 W_2 \int_{-\infty}^{\infty} f_2'(\eta(\Delta e_{i1} + s) + \Delta e_{i2}) f_1(s)\, ds < K_1''(e_{i1}); \tag{A1.9}$$

$$\begin{aligned} &K_1''(e_{i1}) W_2 \int_{-\infty}^{\infty} f_2'(\eta(\Delta e_{i1} + s) + \Delta e_{i2}) f_1(s)\, ds \\ &+K_2''(e_{i2}) \cdot \left[f_1'(\Delta e_{i1}) W_1 + \eta^2 W_2 \int_{-\infty}^{\infty} f_2'(\eta(\Delta e_{i1} + s) + \Delta e_{i2}) f_1(s)\, ds \right] \\ &-f_1'(\Delta e_{i1}) W_1 W_2 \int_{-\infty}^{\infty} f_2'(\eta(\Delta e_{i1} + s) + \Delta e_{i2}) f_1(s)\, ds < K_1''(e_{i1}) K_2''(e_{i2}). \end{aligned} \tag{A1.10}$$

If these conditions hold globally, the expected payoff of player i is a strictly concave function of (e_{i1}, e_{i2}), so that (1.15) and (1.16) describe best responses, and thus characterize a Nash equilibrium. Furthermore, this is the unique symmetric equilibrium.

A1.1.6 Discussing second-order conditions (no revelation)

Global second-order conditions We first show that (A1.9) and (A1.10) hold for given policy parameters and distributions as long as K_t is sufficiently convex for $t = 1, 2$. For (A1.9), this is obvious, as the right-hand side is increasing in $K_1''()$.

To see that the statement is also true for (A1.10), let

$$\begin{aligned} A &\equiv W_2 \int_{-\infty}^{\infty} f_2'(\eta(\Delta e_{i1} + s) + \Delta e_{i2}) f_1(s)\, ds; \\ B &\equiv f_1'(\Delta e_{i1}) W_1 + \eta^2 W_2 \int_{-\infty}^{\infty} f_2'(\eta(\Delta e_{i1} + s) + \Delta e_{i2}) f_1(s)\, ds; \\ C &\equiv -f_1'(\Delta e_{i1}) W_1 W_2 \int_{-\infty}^{\infty} f_2'(\eta(\Delta e_{i1} + s) + \Delta e_{i2}) f_1(s)\, ds. \end{aligned}$$

With this notation, (A1.10) can be written as

$$K_1''(e_{i1}) \cdot A + K_2''(e_{i2}) \cdot B + C \leq K_1''(e_{i1}) K_2''(e_{i2}). \tag{A1.11}$$

To prove that (A1.10) holds for sufficiently convex cost functions, suppose it does not hold for some pair of cost function $\widetilde{K}_1$ and $\widetilde{K}_2$. Let $\widehat{K}_t(e) = \widetilde{K}_t(e) + \frac{\kappa}{2} e^2$. Then (A1.11) for $\widehat{K}_1$ and $\widehat{K}_2$ is

$$\begin{gathered} \widetilde{K}_1''(e_{i1}) \cdot A + \widetilde{K}_2''(e_{i2}) \cdot B + C \leq \\ \widetilde{K}_1''(e_{i1}) \widetilde{K}_2''(e_{i2}) + \kappa \left(\widetilde{K}_1''(e_{i1}) + \widetilde{K}_2''(e_{i2}) - A - B \right) + \kappa^2. \end{gathered}$$

[42](A1.9) is the condition that expected payoffs are strictly concave in e_{i1}; (A1.10) is the condition that the Hessian of the expected payoff function has strictly positive determinant.

For all A and B, the right-hand side of this inequality can be made arbitrarily high by increasing κ, so that the inequality is satisfied and thus (A1.10) holds.

Local second-order conditions In the symmetric equilibrium, $\Delta e_{i1} = \Delta e_{i2} = 0$. Using this equation in (A1.9) and (A1.10), $f_1'(0) = 0$ and the symmetry of f_1 and f_2 (Assumption 1.2) gives

$$\eta^2 W_2 \int_{-\infty}^{\infty} f_2'(\eta s) f_1(s)\, ds < K_1''(e_{i1}); \tag{A1.12}$$

$$\left(\frac{W_2}{K_2''(e_{i2})} + \frac{\eta^2 W_2}{K_1''(e_{i1})} \right) \int_{-\infty}^{\infty} f_2'(\eta s) f_1(s)\, ds \leq 1. \tag{A1.13}$$

By Assumption 1.2, $f_1(s) = f_1(-s)$ and $f_2'(\eta s) = -f_2'(-\eta s)$. This implies that $\int_{-\infty}^{\infty} f_2'(\eta s) f_1(s)\, ds = 0$. Thus, the left-hand sides of (A1.12) and (A1.13) are all 0 and the inequalities hold automatically.

A1.2 Revelation policy: Proof of Proposition 1.3

(1.13) and (1.16) imply

$$\begin{gathered} e_2^*(\eta, W - W_1, 0) - E\left(e_2^*(\eta, W - W_1, 1)\right) = \\ (K_2')^{-1}\left((W - W_1)\, C(\eta)\right) - 2\int_0^{\infty} (K_2')^{-1}\left(f_2(\eta s)(W - W_1)\right) f_1(s)\, ds. \end{gathered}$$

Using Definition 1.3 and the symmetry of f_1 and f_2, the right-hand side can be written as

$$(K_2')^{-1}\left((W - W_1)\int_{-\infty}^{\infty} f_2(\eta s) f_1(s)\, ds\right) - \int_{\infty}^{\infty} (K_2')^{-1}\left(f_2(\eta s)(W - W_1)\right) f_1(s)\, ds.$$

Substituting $g(s) \equiv (W - W_1) f_2(\eta s)$, this becomes

$$(K_2')^{-1}\left(\int_{-\infty}^{\infty} g(s) f_1(s)\, ds\right) - \int_{-\infty}^{\infty} (K_2')^{-1}(g(s)) f_1(s)\, ds.$$

According to Jensen's inequality, this expression is weakly negative (weakly positive) if $(K_2')^{-1}$ is convex (concave), which is the case if and only if K_2' is concave (convex), that is, $K_2''' \leq 0$ ($K_2''' \geq 0$).[43]

[43] The proof resembles Aoyagi (2010) and Ederer (2010) (case with non-complementary abilities), but allows for $W_1 > 0$ and $\eta \neq 1$.

A1.3 Optimal weights

A1.3.1 Proof of Proposition 1.4

We start with several auxiliary results. Then, we show that $\eta < 0$ is never optimal. Finally, we show that it is always optimal to increase η from zero to some positive value.

Lemma A1.2. *Suppose $\eta_1 > 0$ and $\eta_2 = -\eta_1$. Then,*

(i)

$$e_1^*(\eta_1, W - W_1, 1) > e_1^*(\eta_2, W_1, W - W_1).$$

(ii)

$$E(e_2^*(\eta_1, W - W_1, 1)) = E(e_2^*(\eta_2, W - W_1, 1)).$$

(iii)

$$e_2^*(\eta_1, W - W_1, 0) = e_2^*(\eta_2, W - W_1, 0).$$

Proof.

(i) From (1.12) and (1.15) and using (1.10), we have

$$\begin{aligned} e_1^*(\eta_1, W_1, W - W_1) - e_1^*(\eta_2, W_1, W - W_1) = \\ (K_1')^{-1}(f_1(0) W_1 + \eta_1 (W - W_1) C(\eta_1)) \\ - (K_1')^{-1}(f_1(0) W_1 - \eta_1 (W - W_1) C(\eta_1)). \end{aligned} \tag{A1.14}$$

As $K_1'' > 0$, $(K_1')^{-1}$ is strictly increasing. Thus, (A1.14) is strictly positive.

(ii) (1.13) and $f_2(\eta_2 s) = f_2(\eta_1 s)$ imply the result.

(iii) (1.16) and (1.10) imply the result.

□

Lemma A1.3. *Suppose $W_1 < W$. Then,*

(i)

$$\left.\frac{\partial e_1^*(\eta, W_1, W - W_1)}{\partial \eta}\right|_{\eta=0} > 0.$$

(ii)

$$\left.\frac{\partial E(e_2^*(\eta, W - W_1, 1))}{\partial \eta}\right|_{\eta=0} = \left.\frac{\partial e_2^*(\eta, W - W_1, 0)}{\partial \eta}\right|_{\eta=0} = 0.$$

Proof.

(i) From (1.12) and (1.15),

$$\frac{\partial e_1^*(\eta, W_1, W - W_1)}{\partial \eta} = \frac{(W - W_1)(C(\eta) + \eta C'(\eta))}{K_1''\left[(K_1')^{-1}(f_1(0) W_1 + \eta (W - W_1) C(\eta))\right]}. \tag{A1.15}$$

Hence,

$$\left.\frac{\partial e_1^*(\eta, W_1, W - W_1)}{\partial \eta}\right|_{\eta=0} = \frac{(W - W_1) C(0)}{K_1''\left[(K_1')^{-1}(f_1(0) W_1)\right]} = \frac{(W - W_1) f_2(0)}{K_1''\left[(K_1')^{-1} f_1(0) W_1\right]}.$$

where the second equality follows from (1.8). As $K_1'' > 0$ and $f_2(0) > 0$, $\left.\frac{\partial e_1^*(\eta, W_1, W - W_1)}{\partial \eta}\right|_{\eta=0} > 0$ provided $W_1 < W$.

(ii) From (1.13),

$$\frac{\partial E(e_2^*(\eta_1, W - W_1, 1))}{\partial \eta} = 2\int_0^\infty \frac{s f_2'(\eta s)(W - W_1) f_1(s)}{K_2''\left[(K_2')^{-1}(f_2(\eta s)(W - W_1))\right]} ds. \tag{A1.16}$$

Hence,

$$\left.\frac{\partial E(e_2^*(\eta_1, W - W_1, 1))}{\partial \eta}\right|_{\eta=0} = 2\int_0^\infty \frac{s f_2'(0)(W - W_1) f_1(s)}{K_2''\left[(K_2')^{-1}(f_2(0)(W - W_1))\right]} ds = 0,$$

where the second equality follows from $f_2'(0) = 0$. Next, from (1.16),

$$\frac{\partial e_2^*(\eta, W - W_1, 0)}{\partial \eta} = \frac{(W - W_1) C'(\eta)}{K_2''\left[(K_2')^{-1}((W - W_1) C(\eta))\right]}.$$

Hence,

$$\left.\frac{\partial e_2^*(\eta, W - W_1, 0)}{\partial \eta}\right|_{\eta=0} = \frac{(W - W_1) C'(0)}{K_2''\left[(K_2')^{-1}((W - W_1) C(0))\right]} = 0,$$

where the second equality follows from (1.9).

□

To see that $\eta < 0$ is never optimal, note that Lemma (A1.2) (i)-(iii) implies that for every $\eta < 0$, $-\eta > 0$ yields strictly higher first-period efforts and equally high second-period efforts. Thus, for any revelation policy and whether efforts are perfect or imperfect substitutes, the optimal η is non-negative.

To see that for $W_1 < W$ the optimal η is positive, note that by Lemma (A1.3) (i) and (ii), increasing η marginally from zero increases first-period efforts, while there is no effect on second-period efforts. Hence, for any revelation policy and whether efforts are perfect or imperfect substitutes, the optimal η is positive provided $W_1 < W$.

A1.3.2 Proof of Proposition 1.5

From (1.17),

$$\frac{\partial V^P(\eta, W_1, 1)}{\partial \eta} = \frac{\partial e_1^*(\eta, W_1, W - W_1)}{\partial \eta} + \frac{\partial E\left(e_2^*(\eta_1, W - W_1, 1)\right)}{\partial \eta}. \tag{A1.17}$$

Using Assumption 1.3 and (1.7) to simplify (A1.15) and (A1.16), (A1.17) becomes

$$\frac{\partial V^P(\eta, W_1)}{\partial \eta} = \frac{(W - W_1)\left(C(\eta) + \eta C'(\eta)\right)}{k_1} + \frac{(W - W_1)\, C'(\eta)}{k_2}. \tag{A1.18}$$

Solving $\frac{\partial V^P(\eta, W_1)}{\partial \eta} = 0$ and rearranging gives the result.

A1.4 Optimal prize structure for perfect substitutes

First, we provide results on the optimal prize structure for the case of general K_1 and K_2 that are not necessarily quadratic. As the revelation policy matters in this case, we first address the optimal prize structure for the full revelation case in Proposition A1.1. The result will rely on the Assumption that $K_t''' \leq 0$. This is not a serious restriction: Corollary 1.3 states that $K_2''' \leq 0$ is the case in which full revelation is optimal. Second, we consider the no revelation case in Proposition A1.2 for $K_t''' \geq 0$. Again, this is not a serious restriction because for $K_2''' \geq 0$ no revelation is optimal by Corollary 1.3. Third, we derive Corollary 1.4 for $K_t''' = 0$.

A1.4.1 Full revelation

Proposition A1.1. *Suppose $K_t''' \leq 0$ for $t = 1, 2$. For all $\eta > 0$, $W_1^P(\eta, 1) = 0$ ($W_1^P(\eta, 1) = W$) if and only if*

$$\begin{gathered} W f_1(0) < (>) \\ K_1'\left[\left(K_1'\right)^{-1}\left(\eta W C(\eta)\right) + 2\int_0^\infty \left(K_2'\right)^{-1}\left(f_2(\eta s)\, W\right) f_1(s)\, ds\right]. \end{gathered} \tag{A1.19}$$

Proof. Using (1.12) and (1.13) in (1.17) gives

$$\begin{aligned} V^P(\eta, W_1, 1) = {} & (K_1')^{-1}(f_1(0)W_1 + \eta(W - W_1)C(\eta)) \\ & + 2\int_0^\infty (K_2')^{-1}(f_2(\eta s)(W - W_1))f_1(s)\,ds. \end{aligned}$$

This yields

$$\begin{gathered} \frac{\partial V^P(\eta, W_1, 1)}{\partial W_1} = \\ \frac{f_1(0) - \eta C(\eta)}{K_1''\left[(K_1')^{-1}(f_1(0)W_1 + \eta(W - W_1)C(\eta))\right]} \\ -2\int_0^\infty \frac{f_2(\eta s)f_1(s)}{K_2''\left[(K_2')^{-1}(f_2(\eta s)(W - W_1))\right]}ds, \end{gathered}$$

and hence

$$\begin{gathered} \frac{\partial^2 V^P(\eta, W_1, 1)}{(\partial W_1)^2} = \\ -\frac{K_1'''\left[(K_1')^{-1}(f_1(0)W_1 + \eta(W - W_1)C(\eta))\right](f_1(0) - \eta C(\eta))^2}{\left(K_1''\left[(K_1')^{-1}(f_1(0)W_1 + \eta(W - W_1)C(\eta))\right]\right)^3} \\ -2\int_0^\infty \frac{(f_2(\eta s))^2 K_2'''\left[(K_2')^{-1}(f_2(\eta s)(W - W_1))\right]f_1(s)\,ds}{\left(K_2''\left[(K_2')^{-1}(f_2(\eta s)(W - W_1))\right]\right)^3}. \end{gathered}$$

Since $K_t''' > 0$, $K_t''' \leq 0$ implies $\frac{\partial^2 V^P(\eta, W_1, 1)}{(\partial W_1)^2} \geq 0$. Thus, there is no interior optimum. For $W_1 = 0$ and $W_1 = W$, the principal's expected payoffs are

$$\begin{aligned} V^P(\eta, 0, 1) &= (K_1')^{-1}(\eta W C(\eta)) + 2\int_0^\infty (K_2')^{-1}(f_2(\eta s)W)f_1(s)\,ds; \\ V^P(\eta, W, 1) &= (K_1')^{-1}(f_1(0)W). \end{aligned}$$

Therefore,

$$\begin{gathered} V^P(\eta, 0, 1) - V^P(\eta, W, 1) = \\ (K_1')^{-1}(\eta W C(\eta)) + 2\int_0^\infty (K_2')^{-1}(f_2(\eta s)W)f_1(s)\,ds - (K_1')^{-1}(f_1(0)W). \end{gathered}$$

Hence, $V^P(\eta, 0, 1) - V^P(\eta, W, 1) > (<)\ 0$ if and only if (A1.19) holds. $\square$

A1.4.2 No revelation

For the no revelation case, we again restrict the third derivative of the cost functions in such a way that the revelation policy is optimal by Corollary 1.3.

Proposition A1.2. *Suppose $K_t''' \geq 0$ for $t = 1, 2$. For all $\eta > 0$*

(i) $W_1^P(\eta, 0) = 0$ *if*

$$\frac{f_1(0) - \eta C(\eta)}{K_1''\left[(K_1')^{-1}(\eta W C(\eta))\right]} - \frac{C(\eta)}{K_2''\left[(K_2')^{-1}(W C(\eta))\right]} < 0. \tag{A1.20}$$

(ii) $W_1^P(\eta, 0) = W$ *if*

$$\frac{f_1(0) - \eta C(\eta)}{K_1''\left[(K_1')^{-1}(f_1(0) W)\right]} - \frac{C(\eta)}{K_2''(0)} > 0. \tag{A1.21}$$

(iii) If neither (A1.20) *nor* (A1.21) *holds,* $W_1^P \in [0, W]$.

Proof. Using (1.15) and (1.16) in (1.17) gives

$$V^P(\eta, W_1, 0) = \\ (K_1')^{-1}(f_1(0) W_1 + \eta(W - W_1) C(\eta)) + (K_2')^{-1}((W - W_1) C(\eta)).$$

This yields

$$\frac{\partial V^P(\eta, W_1, 0)}{\partial W_1} = \\ \frac{f_1(0) - \eta C(\eta)}{K_1''\left[(K_1')^{-1}(f_1(0) W_1 + \eta(W - W_1) C(\eta))\right]} - \frac{C(\eta)}{K_2''\left[(K_2')^{-1}((W - W_1) C(\eta))\right]}$$

and

$$\frac{\partial^2 V^P(\eta, W_1, 0)}{(\partial W_1)^2} = \\ -\frac{(f_1(0) - \eta C(\eta))^2 \cdot K_1'''\left[(K_1')^{-1}(f_1(0) W_1 + \eta(W - W_1) C(\eta))\right]}{\left(K_1''\left[(K_1')^{-1}(f_1(0) W_1 + \eta(W - W_1) C(\eta))\right]\right)^3} \\ -\frac{(C(\eta))^2 \cdot K_2'''\left[(K_2')^{-1}((W - W_1) C(\eta))\right]}{\left(K_2''\left[(K_2')^{-1}((W - W_1) C(\eta))\right]\right)^3}.$$

Since $K_t'' > 0$, $K_t''' \geq 0$ implies $\frac{\partial^2 V^P(\eta, W_1, 0)}{(\partial W_1)^2} \leq 0$.

(i) Thus, the principal will set $W_1 = 0$ provided

$$\left.\frac{\partial V^P(\eta, W_1, 0)}{\partial W_1}\right|_{W_1=0} = \frac{f_1(0) - \eta C(\eta)}{K_1''\left[(K_1')^{-1}(\eta W C(\eta))\right]} - \frac{C(\eta)}{K_2''\left[(K_2')^{-1}(W C(\eta))\right]} < 0.$$

(ii) She will set $W_1 = W$ provided

$$\left.\frac{\partial V^P(\eta, W_1, 0)}{\partial W_1}\right|_{W_1=W} = \frac{f_1(0) - \eta C(\eta)}{K_1''\left[(K_1')^{-1}(f_1(0) W)\right]} - \frac{C(\eta)}{K_2''(0)} > 0.$$

□

A1.4.3 Proof of Corollary 1.4

Proof of part (i). With $K_t''' = 0$, Proposition A1.1 implies that $W_1^P(\eta)$ is always a boundary solution, with $W_1^P(\eta) = 0$ if $\frac{1}{k_1} f_1(0) W < \left(\frac{\eta}{k_1} + \frac{1}{k_2}\right) W C(\eta)$. This gives the first result. Note that the left-hand side of the last equation is total effort for $W_1 = W$, while the right-hand side is total effort for some η and $W_1 = 0$. Hence, if the inequality is satisfied for some η, then total effort for this η and $W_1 = 0$ is higher than for $W_1 = W$, which shows that $W_1 = W$ cannot be optimal. In this case, since there is no interior optimum by Proposition A1.1, $W_1 = 0$ is optimal.[44]

Proof of part (ii). Analogous.

A1.5 Optimal prize structure for imperfect substitutes

A1.5.1 Proof of Proposition 1.6

Using (1.12) and (1.13) in (1.18) yields

$$\begin{aligned} V^I(\eta_1, W_1, 1) = {} & (K_1')^{-1}(f_1(0) W_1 + \eta (W - W_1) C(\eta)) \cdot \\ & 2\int_0^\infty (K_2')^{-1}(f_2(\eta s)(W - W_1)) f_1(s)\, ds. \end{aligned} \tag{A1.22}$$

[44] This can also be derived from Proposition A1.2.

Using (A1.22), we have

$$\frac{\partial V^I(\eta, W_1, 1)}{\partial W_1} = \frac{2\left(f_1(0) - \eta C(\eta)\right) \int_0^\infty \left(K_2'\right)^{-1}\left(f_2(\eta s)(W - W_1)\right) f_1(s)\, ds}{K_1''\left[\left(K_1'\right)^{-1}\left(f_1(0) W_1 + \eta (W - W_1) C(\eta)\right)\right]}$$
$$-2\left(K_1'\right)^{-1}\left(f_1(0) W_1 + \eta (W - W_1) C(\eta)\right) \cdot \int_0^\infty \frac{f_2(\eta s)}{K_2''\left[\left(K_2'\right)^{-1}\left(f_2(\eta s)(W - W_1)\right)\right]} f_1(s)\, ds.$$

Thus,

$$\frac{\partial^2 V^I(\eta, W_1, 1)}{(\partial W_1)^2} = -\frac{2\left(f_1(0) - \eta C(\eta)\right)^2 \cdot K_1'''\left[\left(K_1'\right)^{-1}\left(f_1(0) W_1 + \eta (W - W_1) C(\eta)\right)\right]}{\left(K_1''\left[\left(K_1'\right)^{-1}\left(f_1(0) W_1 + \eta (W - W_1) C(\eta)\right)\right]\right)^3} \cdot \int_0^\infty \left(K_2'\right)^{-1}\left(f_2(\eta s)(W - W_1)\right) f_1(s)\, ds$$
$$-\frac{4\left(f_1(0) - \eta C(\eta)\right)}{K_1''\left[\left(K_1'\right)^{-1}\left(f_1(0) W_1 + \eta (W - W_1) C(\eta)\right)\right]} \int_0^\infty \frac{f_2(\eta s) f_1(s)\, ds}{K_2''\left[\left(K_2'\right)^{-1}\left(f_2(\eta s)(W - W_1)\right)\right]}$$
$$-\left(K_1'\right)^{-1}\left(f_1(0) W_1 + \eta (W - W_1) C(\eta)\right) \cdot 2 \int_0^\infty \frac{\left(f_2(\eta s)\right)^2}{\left(K_2''\left[\left(K_2'\right)^{-1}\left(f_2(\eta s)(W - W_1)\right)\right]\right)^3} \cdot K_2'''\left[\left(K_2'\right)^{-1}\left(f_2(\eta s)(W - W_1)\right)\right] f_1(s)\, ds.$$

Since $V^I(\eta, W, 1) = 0$, $W_1 = W$ is never optimal. Since $K_t'' > 0$, $K_t''' \leq 0$ implies $\frac{\partial^2 V^I(\eta, W_1, 1)}{(\partial W_1)^2} > 0$ if $f_1(0) - \eta C(\eta) < 0$. For this case, there is no interior optimum and thus $W_1 = 0$.

A1.5.2 Proof of Proposition 1.7

With Assumption 1.3, (A1.22) yields

$$V^I(\eta, W_1) = \frac{(W - W_1) C(\eta)}{k_1 k_2}\left(f_1(0) W_1 + \eta (W - W_1) C(\eta)\right). \tag{A1.23}$$

Thus

$$\frac{\partial V^I(\eta, W_1)}{\partial W_1} = \frac{C(\eta)}{k_1 k_2}\left[f_1(0)(W - 2W_1) - 2\eta C(\eta)(W - W_1)\right]. \tag{A1.24}$$

Because $\eta > 0$, $C(\eta) > 0$ and $k_t > 0$, $W_1 > \frac{1}{2}W$ implies $\frac{\partial V^I(\eta, W_1)}{\partial W_1} < 0$. Hence, $W_1 < \frac{W}{2}$ $\forall$ η.

A1.5.3 Proof of Proposition 1.8

Proof of part (i). Clearly $W_1 > 0$ at the optimum if $\left.\frac{\partial V^I(\eta, W_1)}{\partial W_1}\right|_{W_1=0} > 0$, that is, using (A1.24), if $f_1(0) > 2\eta C(\eta)$. To see that $W_1 = 0$ at the optimum if $\left.\frac{\partial V^I(\eta, W_1)}{\partial W_1}\right|_{W_1=0} < 0$ or, equivalently, $f_1(0) < 2\eta C(\eta)$, first note that (A1.24) implies

$$\frac{\partial^2 V^I(\eta, W_1)}{(\partial W_1)^2} = \frac{C(\eta)}{k_1 k_2}\left[-2f_1(0) + 2\eta C(\eta)\right].$$

Thus $\frac{\partial V^I(\eta, W_1)}{\partial W_1}$ is monotone in W_1. Moreover, according to the proof of Proposition 1.7, $\frac{\partial V^I(\eta, W_1)}{\partial W_1} < 0$ $\forall$ $W_1 > \frac{W}{2}$. The last two statements imply that, whenever $\left.\frac{\partial V^I(\eta, W_1)}{\partial W_1}\right|_{W_1=0} < 0$, then $\frac{\partial V^I(\eta, W_1)}{\partial W_1} < 0$ for all $W_1 \leq \frac{W}{2}$ and thus $W_1^I(\eta) = 0$. To see that $W_1 = 0$ at the optimum if $\left.\frac{\partial V^I(\eta, W_1)}{\partial W_1}\right|_{W_1=0} = 0$ or, equivalently, $f_1(0) = 2\eta C(\eta)$, note that $f_1(0) = 2\eta C(\eta)$ implies $\frac{\partial^2 V^I(\eta, W_1)}{(\partial W_1)^2} < 0$, so that $\frac{\partial V^I(\eta, W_1)}{\partial W_1} < 0$ $\forall$ $W_1 > 0$ and thus $W_1^I(\eta) = 0$.

For $\left.\frac{\partial V^I(\eta, W_1)}{\partial W_1}\right|_{W_1=0} > 0$, the first-order condition $\frac{\partial V^I(\eta, W_1)}{\partial W_1} = 0$ yields $W_1^I(\eta) = W\frac{f_1(0) - 2\eta C(\eta)}{2f_1(0) - 2\eta C(\eta)} > 0$ for $f_1(0) > 2\eta C(\eta)$. Summing up, we obtain

$$W_1^I(\eta) = \begin{cases} W\frac{f_1(0) - 2\eta C(\eta)}{2f_1(0) - 2\eta C(\eta)} > 0, & f_1(0) > 2\eta C(\eta); \\ 0, & f_1(0) \leq 2\eta C(\eta). \end{cases} \tag{A1.25}$$

Proof of part (ii). (A1.25) shows that W_1^I must correspond to one of the two cases mentioned in the proposition. To complete the proof, we derive the first-order condition for $\eta^I(W_1)$ for these two cases. From (A1.23), we obtain

$$\begin{aligned} \frac{\partial V^I(\eta, W_1)}{\partial \eta} = {} & \frac{(W - W_1)^2 C(\eta)}{k_1 k_2}\left(C(\eta) + \eta C'(\eta)\right) \\ & + \frac{(W - W_1) C'(\eta)}{k_1 k_2}\left(f_1(0) W_1 + \eta (W - W_1) C(\eta)\right). \end{aligned}$$

The first-order condition $\frac{\partial V^I(\eta, W_1)}{\partial \eta} = 0$ yields

$$W_1 = W\frac{(C(\eta))^2 + 2\eta C(\eta) C'(\eta)}{C(\eta)^2 + 2\eta C(\eta) C'(\eta) - f_1(0) C'(\eta)}. \tag{A1.26}$$

According to (A1.25), $W_1 = 0$ is a necessary condition for an optimum with $f_1(0) \leq 2\eta C(\eta)$. Inserting $W_1 = 0$ in (A1.26) gives the first-order condition $\eta = -\frac{C(\eta)}{2C'(\eta)}$, which corresponds to Proposition 1.8 (ii) (a). Analogously, $W_1 = W\frac{f_1(0)-2\eta C(\eta)}{2f_1(0)-2\eta C(\eta)}$ is a necessary condition for an optimum with $f_1(0) > 2\eta C(\eta)$. Inserting $W_1 = W\frac{f_1(0)-2\eta C(\eta)}{2f_1(0)-2\eta C(\eta)}$ in (A1.26) and solving for $f_1(0)$ gives the first-order condition $f_1(0) = -\frac{(C(\eta))^2}{C'(\eta)}$, which corresponds to Proposition 1.8 (ii) (b).

A1.6 The normal-quadratic example E1

A1.6.1 Proof of Corollary 1.5

We will proceed in three steps. First, we will provide several auxiliary results. Next, we will derive sufficient conditions for the second-order conditions to hold.[45] Finally, we will characterize the equilibrium.

(i) Auxiliary results Note that $\frac{2}{\sqrt{\pi}}\int_0^x \exp(-t^2)\,dt$ is the error function, for which

$$\frac{2}{\sqrt{\pi}}\int_0^\infty \exp\left(-t^2\right)dt = 1. \tag{A1.27}$$

Next, for E1,

$$f_t(s) = \frac{1}{\sigma_t\sqrt{2\pi}}\exp\left(-\frac{s^2}{2\sigma_t^2}\right). \tag{A1.28}$$

Hence,

$$\begin{aligned} f_t'(s) &= -\frac{1}{\sqrt{2\pi}}\frac{s}{\sigma_t^3}\exp\left(-\frac{s^2}{2\sigma_t^2}\right); \\ f_t''(s) &= \frac{1}{\sqrt{2\pi}}\frac{s^2-\sigma_t^2}{\sigma_t^5}\exp\left(-\frac{s^2}{2\sigma_t^2}\right); \\ f_t'''(s) &= \frac{1}{\sqrt{2\pi}}\frac{3\sigma_t^2 s - s^3}{\sigma_t^7}\exp\left(-\frac{s^2}{2\sigma_t^2}\right). \end{aligned} \tag{A1.29}$$

As $s = -\sigma_t$ (the solution to $f_t''(s) = 0$ and $f_t'''(s) < 0$) maximizes $f_t'(x)$, we obtain $\forall\, x \in \mathbb{R}\ f_t'(x) \leq -\frac{1}{\sqrt{2\pi}}\frac{-\sigma_t}{\sigma_t^3}\exp\left(-\frac{\sigma_t^2}{2\sigma_t^2}\right)$ and thus

$$f_t'(x) \leq \frac{1}{\sigma_t^2\sqrt{2\pi\exp(1)}}. \tag{A1.30}$$

Furthermore, (A1.28) implies

$$\int_0^\infty f_2(\eta s)\,ds = \frac{1}{\sigma_2\sqrt{2\pi}}\int_0^\infty \exp\left(-\left(\frac{s\eta}{\sqrt{2}\sigma_2}\right)^2\right)ds.$$

[45] We only consider the second-order conditions for the full revelation case.

Substituting $s = \frac{\sqrt{2}\sigma_2}{|\eta|}t$ and $ds = \frac{\sqrt{2}\sigma_2}{|\eta|}dt$ implies

$$\int_0^\infty f_2(\eta s)\, ds = \frac{1}{|\eta|\sqrt{\pi}} \int_0^\infty \exp\left(-t^2\right) dt.$$

With (A1.27), we get

$$\int_0^\infty f_2(\eta s)\, ds = \frac{1}{2|\eta|}. \tag{A1.31}$$

Next, (A1.28) and (A1.29) imply

$$\int_0^\infty f_2(\eta s)\, f_2'(\eta s)\, ds = -\frac{\eta}{2\pi\sigma_2^4} \int_0^\infty s \cdot \exp\left(-\frac{s^2\eta^2}{\sigma_2^2}\right) ds.$$

Substituting $s = \frac{\sqrt{t}\sigma_2}{|\eta|}$ and $ds = \frac{\sigma_2}{2\sqrt{t}|\eta|}dt$ and noting that $\int_0^\infty \exp(-t)\, dt = 1$, we obtain

$$\int_0^\infty f_2(\eta s)\, f_2'(\eta s)\, ds = -\frac{1}{4\pi\eta\sigma_2^2}. \tag{A1.32}$$

Furthermore, (A1.28) implies

$$C(\eta) = \frac{1}{\pi\sigma_1\sigma_2} \int_0^\infty \exp\left(-\left(s\frac{\sqrt{\sigma_1^2\eta^2+\sigma_2^2}}{\sqrt{2}\sigma_1\sigma_2}\right)^2\right) ds.$$

Substituting $s = \frac{\sqrt{2}\sigma_1\sigma_2}{\sqrt{\sigma_1^2\eta^2+\sigma_2^2}}t$ and $ds = \frac{\sqrt{2}\sigma_1\sigma_2}{\sqrt{\sigma_1^2\eta^2+\sigma_2^2}}dt$ yields

$$C(\eta) = \frac{\sqrt{2}}{\pi\sqrt{\sigma_1^2\eta^2+\sigma_2^2}} \int_0^\infty \exp\left(-t^2\right) dt.$$

With (A1.27), we get

$$C(\eta) = \frac{1}{\sqrt{2\pi}\sqrt{\sigma_1^2\eta^2+\sigma_2^2}}, \tag{A1.33}$$

so that

$$C'(\eta) = -\frac{\eta\sigma_1^2}{\sqrt{2\pi}\left(\sigma_1^2\eta^2+\sigma_2^2\right)^{\frac{3}{2}}}. \tag{A1.34}$$

(ii) Second-order conditions Using $K_t''(e_{it}) = k$, (A1.1) simplifies to

$$f_2'(x)\, W_2 < k \;\forall\; x \in \mathbb{R}. \tag{A1.35}$$

From $W_2 \leq W$ and (A1.30),

$$f_2'(x)\, W_2 \leq \frac{W}{\sigma_2^2\sqrt{2\pi\exp(1)}}. \tag{A1.36}$$

(A1.35) and (A1.36) imply that a sufficient condition for (A1.1) to hold is

$$k > \frac{W}{\sigma_2^2 \sqrt{2\pi \exp(1)}}. \tag{A1.37}$$

Similarly, (A1.8) can be written as

$$\begin{aligned} & f_1'(x) W_1 + \eta W_2 \int_0^\infty f_2(\eta s) \left[f_1'(s+x) - f_1'(s-x)\right] ds \\ & + \frac{\eta W_2^2}{k} \int_0^\infty f_2(\eta s) f_2'(\eta s) \left[f_1'(s+x) + f_1'(s-x)\right] ds < k. \end{aligned} \tag{A1.38}$$

Using (A1.30), we obtain $\forall\ x \in \mathbb{R}$

$$\begin{aligned} f_1'(x) W_1 &\le \frac{W_1}{\sigma_1^2 \sqrt{2\pi \exp(1)}}; \\ \eta W_2 \int_0^\infty f_2(\eta s) \left[f_1'(s+x) - f_1'(s-x)\right] ds &\le \frac{2W_2 \left|\eta \int_0^\infty f_2(\eta s)\, ds\right|}{\sigma_1^2 \sqrt{2\pi \exp(1)}}; \\ \frac{\eta W_2^2}{k} \int_0^\infty f_2(\eta s) f_2'(\eta s) \left[f_1'(s+x) + f_1'(s-x)\right] ds &\le \frac{2W_2^2 \left|\eta \int_0^\infty f_2(\eta s) f_2'(\eta s)\, ds\right|}{k\sigma_1^2 \sqrt{2\pi \exp(1)}}. \end{aligned}$$

This yields an upper bound for the left-hand side of (A1.38):

$$\frac{1}{\sigma_1^2 \sqrt{2\pi \exp(1)}} \left[W_1 + 2W_2 \left|\eta \int_0^\infty f_2(\eta s)\, ds\right| + \frac{2W_2^2}{k} \left|\eta \int_0^\infty f_2(\eta s) f_2'(\eta s)\, ds\right|\right].$$

With (A1.31) and (A1.32), this upper bound can be written as

$$\frac{W_1 + W_2}{\sigma_1^2 \sqrt{2\pi \exp(1)}} + \frac{W_2^2}{k\sigma_1^2 \sigma_2^2 (2\pi)^{\frac{3}{2}} \sqrt{\exp(1)}} \le \frac{W}{\sigma_1^2 \sqrt{2\pi \exp(1)}} + \frac{W^2}{k\sigma_1^2 \sigma_2^2 (2\pi)^{\frac{3}{2}} \sqrt{\exp(1)}}.$$

A sufficient condition for (A1.8) to hold is thus

$$k > \frac{W}{\sigma_1^2 \sqrt{2\pi \exp(1)}} + \frac{W^2}{k\sigma_1^2 \sigma_2^2 (2\pi)^{\frac{3}{2}} \sqrt{\exp(1)}}. \tag{A1.39}$$

(iii) Characterizing the equilibrium Proposition 1.1 thus characterizes the PBE. As $K_2''' = 0$, Proposition 1.3 implies that efforts under both revelation policies are equal in expected value. Inserting $(K_t')^{-1}(x) = \frac{x}{k}$, (A1.28) and (A1.33) in (1.12) and (1.13) yields (1.20) and (1.21).

A1.6.2 Proof of Corollary 1.6

Proof of part (i). We first derive $\eta^P(W_1)$. With (A1.33) and (A1.34), we obtain

$$\frac{C'(\eta)}{C(\eta)} = -\frac{\eta\sigma_1^2}{\sigma_2^2 + \sigma_1^2\eta^2}.$$

Proposition 1.5 (i) thus implies $\frac{\eta\sigma_1^2}{\sigma_2^2+\sigma_1^2\eta^2} = \frac{1}{1+\eta}$ as a necessary condition, which is uniquely (and positively) solved by $\eta = \frac{\sigma_2^2}{\sigma_1^2} > 0$. Since the optimal η must be strictly positive by Proposition 1.4 and since the solution to the necessary condition is unique and positive, the necessary condition is sufficient and we have $\eta^P(W_1) = \frac{\sigma_2^2}{\sigma_1^2}\ \forall\ W_1 < W$. Next, we show that $W_1^P = 0$. By Corollary 1.4, $W_1^P = 0$ if $\exists\ \eta$ such that $f_1(0) < (1+\eta)\, C(\eta)$. From (A1.28) and (A1.33), this condition is equivalent to

$$\frac{1}{\sigma_1\sqrt{2\pi}} < \frac{1+\eta}{\sqrt{2\pi}\sqrt{\sigma_1^2\eta^2+\sigma_2^2}} \Longleftrightarrow \eta > \frac{\frac{\sigma_2^2}{\sigma_1^2} - 1}{2}.$$

In particular, this holds for $\eta^P(W_1) = \frac{\sigma_2^2}{\sigma_1^2}$. Hence, $W_1^P = 0$ and $\eta^P = \eta^P\left(W_1^P\right) = \frac{\sigma_2^2}{\sigma_1^2}$.

Proof of part (ii). (A1.33) and (A1.34) yield

$$C(\eta)^2 + C'(\eta)\, f_1(0) = \frac{\sqrt{\eta^2\sigma_1^2 + \sigma_2^2} - \eta\sigma_1}{2\pi\left(\eta^2\sigma_1^2+\sigma_2^2\right)^{\frac{3}{2}}} > 0\ \forall\ \eta.$$

This is inconsistent with $\left|\frac{C'(\eta)}{C(\eta)}\right| = \frac{C(\eta)}{f_1(0)}$ as for $\eta > 0$, $\left|\frac{C'(\eta)}{C(\eta)}\right| = \frac{C(\eta)}{f_1(0)}$ is equivalent to $C(\eta)^2 + C'(\eta)\, f_1(0) = 0$. Therefore, according to Proposition 1.8 (ii) (b), $W_1^I > 0$ cannot apply. Hence, Proposition (1.8) (ii) gives

$$\left|\frac{C'(\eta)}{C(\eta)}\right| = \frac{1}{2\eta}$$

as the necessary condition for η^I. Using (A1.33) and (A1.34), this can be written as

$$\frac{\eta\sigma_1^2}{\sigma_2^2 + \sigma_1^2\eta^2} = \frac{1}{2\eta},$$

which is solved by $\eta^I = \frac{\sigma_2}{\sigma_1}$.

References

Aoyagi, M. (2010). Information feedback in a dynamic tournament. *Games and Economic Behavior*, 70(2), 242–260.

Arbatskaya, M. & Mialon, H. M. (2012). Dynamic multi-activity contests. *The Scandinavian Journal of Economics*, 114(2), 520–538.

Baik, K. H. & Lee, S. (2000). Two-stage rent-seeking contests with carryovers. *Public Choice*, 103(3-4), 285–296.

Casas-Arce, P. & Martínez-Jerez, F. A. (2009). Relative performance compensation, contests, and dynamic incentives. *Management Science*, 55(8), 1306–1320.

Chen, B. R. & Chiu, Y. S. (2013). Interim performance evaluation in contract design. *The Economic Journal*, 123(569), 665–698.

Clark, D. J. & Konrad, K. A. (2007). Contests with multi-tasking. *The Scandinavian Journal of Economics*, 109(2), 303–319.

Clark, D. J. & Nilssen, T. (2013). Learning by doing in contests. *Public Choice*, 156(1-2), 329–343.

Clark, D. J., Nilssen, T., & Sand, J. Y. (2012). Motivating over time: Dynamic win effects in sequential contests. University of Oslo, Department of Economics Working Paper No. 28/2012.

Denter, P. & Sisak, D. (2013). Do polls create momentum in political competition? Tinbergen Institute Discussion Paper No. 13-169/VII.

Ederer, F. (2010). Feedback and motivation in dynamic tournaments. *Journal of Economics & Management Strategy*, 19(3), 733–769.

Ederer, F. & Fehr, E. (2007). Deception and incentives: How dishonesty undermines effort provision. IZA Discussion Paper No. 3200.

Franckx, L., D'Amato, A., & Brose, I. (2004). Multitask rank order tournaments. *Economics Bulletin*, 10(10), 1–10.

Gershkov, A. & Perry, M. (2009). Tournaments with midterm reviews. *Games and Economic Behavior*, 66(1), 162–190.

Goltsman, M. & Mukherjee, A. (2011). Interim performance feedback in multistage tournaments: The optimality of partial disclosure. *Journal of Labor Economics*, 29(2), 229–265.

Grossmann, M. (2011). Endogenous liquidity constraints in a dynamic contest. University of Zurich, Institute for Strategy and Business Economics Working Paper No. 148.

Grossmann, M. & Dietl, H. M. (2009). Investment behaviour in a two-period contest model. *Journal of Institutional and Theoretical Economics*, 165(3), 401–417.

Hansen, S. E. (2013). Performance feedback with career concerns. *The Journal of Law, Economics, and Organization*, 29(6), 1279–1316.

Harbaugh, R. & Ridlon, R. W. (2010). Handicapping under uncertainty in an all-pay auction. Working paper. Retrieved 2015/01/15, from http://ssrn.com/abstract=1688160.

Hirata, D. (2014). A model of a two-stage all-pay auction. *Mathematical Social Sciences*, 68, 5–13.

Ke, R., Li, J., & Powell, M. (2014). Managing careers in organisations. Unpublished working paper.

Konrad, K. A. (2009). *Strategy and Dynamics in Contests*. New York: Oxford University Press.

Konrad, K. A. & Kovenock, D. (2009). Multi-battle contests. *Games and Economic Behavior*, 66(1), 256–274.

Kovenock, D. & Roberson, B. (2010). Conflicts with multiple battlefields. CESifo Working Paper No. 3165.

Krumer, A. (2013). Best-of-two contests with psychological effects. *Theory and Decision*, 75(1), 85–100.

Lewis, T. R. & Sappington, D. E. M. (1997). Penalizing success in dynamic incentive contracts: No good deed goes unpunished? *The RAND Journal of Economics*, 28(2), 346–358.

Marinovic, I. (2014). The credibility of performance feedback in tournaments. Stanford University, Rock Center for Corporate Governance Working Paper No. 193.

Mas-Colell, A., Whinston, M. D., & Green, J. (1995). *Microeconomic Theory*. New York: Oxford University Press.

Meyer, M. A. (1992). Biased contests and moral hazard: Implications for career profiles. *Annales d'Économie et de Statistique*, 25/26, 165–187.

Moldovanu, B. & Sela, A. (2006). Contest architecture. *Journal of Economic Theory*, 126(1), 70–96.

Möller, M. (2012). Incentives versus competitive balance. *Economics Letters*, 117(2), 505–508.

Nitzan, S. (1994). Modelling rent-seeking contests. *European Journal of Political Economy*, 10(1), 41–60.

Ridlon, R. & Shin, J. (2013). Favoring the winner or loser in repeated contests. *Marketing Science*, 32(5), 768–785.

Schmitt, P., Shupp, R., Swope, K., & Cadigan, J. (2004). Multi-period rent-seeking contests with carryover: Theory and experimental evidence. *Economics of Governance*, 5(3), 187–211.

Sela, A. (2011). Best-of-three all-pay auctions. *Economics Letters*, 112(1), 67–70.

Yildirim, H. (2005). Contests with multiple rounds. *Games and Economic Behavior*, 51(1), 213–227.

Zhang, J. & Wang, R. (2009). The role of information revelation in elimination contests. *The Economic Journal*, 119(536), 613–641.

Chapter 2

Motivating Workers in Dynamic Tournaments – Experimental Evidence

2.1 Introduction

Employees at General Electric in the 1980s and 1990s had a tough life: Every year, supervisors ranked their personnel according to their relative performance into the best 20%, the middle 70%, and the worst 10% of their team. This ranking provided a basis for bonuses, promotions, and even firing decisions (Welch, 2005). While the practice of imposing a fixed distribution upon workers has since been criticized and replaced[1] by more flexible mechanisms, the use of relative incentives – or tournaments – for bonuses and promotions is still widespread within companies (see, for example, Chlosta et al., 2014). Furthermore, tournaments also play a crucial role outside of a human resources context, such as in the assignment of research grants or in incentivizing the development of new ideas (see the examples cited in Morgan & Wang, 2010; Letina & Schmutzler, 2014).

As the example of General Electric implies, an important feature of such tournaments is that they are often repeated over time. Indeed, firms usually operate over several years, making their workers compete against each other multiple times. This generates a series of performance signals among the employees. From the perspective of a firm, this dynamic nature of tournaments poses at least two questions: (1) Should it take past performance signals into account when rewarding the employees today? In other words, should the firm consider the performance history of its employees when distributing bonuses and making promotion decisions, or should

[1]See McGregor (2013) for an overview on recent developments.

it forget about the past and only consider the most recent performance signals? And (2) how should it spread the money for rewards over the different periods?[2] Should rewards have the same size over time, or should they increase or decrease?

The goal of this study is to shed light on these questions from an experimental perspective. To this end, I consider a principal who uses two consecutive tournaments to induce efforts among agents, and has a fixed budget for prizes in these two periods. Using the results from Klein and Schmutzler (2014), I derive testable predictions for the optimal policy in this set-up in both dimensions (1) and (2), the weight of first-period performance in the second tournament and the spread of prize money across periods. I then use a laboratory experiment to test whether the optimal policy increases efforts as predicted. Since actual behavior in tournaments very often differs from theoretical predictions (see, for example, Dechenaux et al., 2012), such empirical evidence is an important complement to theoretical arguments in the design of real-life working environments. Due to the limited observability of effort in the field, where it is only possible to measure performance as a noisy function of effort (Dechenaux et al., 2012, p. 3), laboratory experiments are a particularly useful tool in this empirical analysis.

To derive the theoretical predictions, I consider a special case of the more general model of Klein and Schmutzler (2014). I focus on the maximization of expected total efforts, i.e. the sum of efforts across periods.[3] Furthermore, I consider a particular parameterization of their model, which is easy to implement in the laboratory. Moreover, this parameterization yields intriguingly simple closed-form solutions for the optimal choice of the policy parameters: First, it is optimal to weigh past performance just as strongly as current performance when assigning the second-period prize, independently of how the prize money is spread across periods. Second, the optimal spread of the prize money is such that for all positive weights of past performance in the second-period prize, the principal should shift all the available prize money to the second period. Hence, and third, when setting both policy parameters optimally, the principal shifts all the available prize money to the second period, and weighs past performance just as strongly as current performance in the assignment of the second-period prize. Thus, in the optimal policy, the principal assigns one large prize to the agent who performs best across both periods. As I will argue below, several theoretical and experimental papers on dynamic tournaments rely on a very similar structure without further justification.

[2]Another dimension concerns the revelation of performance information to the employees. See Klein and Schmutzler (2014) for results and a review of further literature on this issue.

[3]Klein and Schmutzler (2014) also consider the maximization of the expected product of efforts across periods.

The experiment introduces the optimal policy stepwise. It consists of the following treatments:

- the baseline policy with two independent but otherwise equal tournaments
- the partially optimal policy, in which the prize in the second tournament goes to the participant with highest sum of performance over both periods
- the globally optimal policy, in which there is only one prize that goes to the participant with highest sum of performance over both periods

The baseline policy captures the situation of a principal who rewards the best-performing agent in each period with an equally sized prize, therefore only taking current performance into account. The partially optimal policy weighs first-period performance as strongly as second-period performance in the assignment of the second-period prize, thus implementing the optimal weight of first-period performance. The globally optimal policy additionally shifts all the available prize money to the second period, and therefore implements both the optimal weight of first-period performance and the optimal spread of the prize money.

This design allows me to identify the effects of adjusting the policy parameters towards the optimal solutions. First, I obtain the effect of adjusting the weight optimally from the baseline to the partially optimal policy (the *weight adjustment*). Furthermore, I can identify the effect of jointly adjusting the weight and the prizes optimally from the baseline to the globally optimal policy (the *weight-prize adjustment*). Finally, I observe the effect of adjusting the prizes optimally, conditional on setting the weight optimally, from the partially to the globally optimal policy (the *prize adjustment*). Klein and Schmutzler (2014) predict that relative to the baseline policy, total efforts increase by about 20% through the weight adjustment and by about 41% through the weight-prize adjustment. The prediction for the increase through the prize adjustment is 17% relative to the partially optimal policy.

The results suggest that the predictive power of the theoretical model is mixed. Although the weight-prize adjustment successfully increases total efforts, its effect is only about 10%, which is a fourth of the predicted increase. The weight adjustment does not increase total efforts. However, all policy adjustments affect the behavior of the participants and change the distribution of efforts across periods. The directions of these changes correspond widely to the predictions of the theoretical model. In particular, the behavior of the participants is in line with the prediction that the weight adjustment increases first- and decreases second-period efforts. On the other hand, I observe substantial over-expenditure of total effort under all three policies. However, over-expenditure decreases with the adjustment of the policy parameters towards the optimum. Finally, there is a large degree of heterogeneity in behavior across participants that contrasts with the prediction of symmetric efforts.

The measures from a pre-experimental questionnaire completed eight days ahead of the laboratory experiment explain parts of the variation in behavior across participants. Under the baseline policy, efforts are decreasing in the participants' degree of loss-aversion. Furthermore, under the globally optimal policy, a prosocial orientation towards other participants decreases efforts. I then argue that one reason for the smaller effect of the weight-prize adjustment compared to the prediction is that it induces particularly low efforts among prosocial participants. Furthermore, I conclude that the policy identified as optimal by the theoretical model may not be the optimal policy for real-world incentive systems.

I also analyze the effect of first-period asymmetry on second-period efforts. Under the baseline policy, the weight of past performance is zero, so that first-period asymmetry should have no effect on second-period behavior. In contrast to this prediction, the tournaments have a dynamic nature for the participants also under the baseline policy. I show that this is driven by prosocial participants who exert more effort when they lost in the first period and less effort when they won. This is consistent with inequality aversion. On the other hand, under the partially and the globally optimal policy, the behavior of prosocial participants does not differ from that of non-prosocial participants. In line with the prediction of the theoretical model, a higher size of first-period asymmetry reduces second-period efforts. This suggests that the monetary link between first and second period through the weight adjustment mitigates the effect of inequality aversion on behavior under the baseline policy. Under the globally optimal policy, I additionally identify a demotivation effect of being behind. A negative performance difference, implying that the participant had lower performance in the first period, is more detrimental to efforts than an equally sized positive performance difference.

This study provides insights which are relevant to the design of competitive incentive systems in practice. Clearly, the precise nature of the optimal incentives for a less stylized setting – i.e. with more periods and agents – is likely to differ from the optimal policy analyzed in this study. Nevertheless, my results suggest that the timing of incentives matters: I find that the weight of past performance and the spread of prize money across periods affect individual behavior. This means that both are important parameters to consider when designing competitive incentives in a repeated setting.

Furthermore, my findings emphasize the relevance of theoretical arguments for the design of incentive systems. In particular, the results imply that comparative statics predictions obtained from simple material utility models may be correct in spite of potential countervailing behavioral phenomena. However, my results also suggest that practitioners should not blindly rely on guidance from simple material utility models in the design of relative incentives. I observe that the globally optimal policy, which generates a high degree of ex-post income inequality, induces particularly low efforts among prosocial participants. This suggests that practitioners should examine relative incentives also regarding their distributive effects and potential consequences for workers with social preferences. Failing to do so may not only lead to wrong predictions of policy effects. It may as well lead to policies which are less efficient than possible.

The study also contributes to several strands of the experimental and theoretical literature on dynamic tournaments. First of all, it complements theoretical work that relies on various types of tournament models and shows a beneficial effect of a positive weight of past performance on effort provision (Meyer, 1992; Gershkov & Perry, 2009; Ridlon & Shin, 2013; Klein & Schmutzler, 2014).[4] Since I find that only an optimal adjustment of both the weight and the prize spread increases efforts, an independent analysis of the optimal weight without considering the optimal prize spread, as in Meyer (1992) and Ridlon and Shin (2013), may be misleading. This justifies the simultaneous analysis of both dimensions by Gershkov and Perry (2009) and Klein and Schmutzler (2014).

Second, my results provide a rationale for the dynamic tournament structure in a series of theoretical and experimental papers on the optimal revelation of past performance (Ederer & Fehr, 2007; Ederer, 2010; Aoyagi, 2010; Ludwig & Lünser, 2012). These papers rely on the assumption that there is only one prize for the agent with highest sum of performance over two periods, which corresponds to the globally optimal policy in this study. My results show that the globally optimal policy performs better than other policies in inducing efforts, providing a justification for this assumption.

Third, the study extends the experimental literature on the effects of the prize structure in tournaments. Previous papers focus on the number of prizes and/or the spread between prizes in one-stage tournaments (Harbring & Irlenbusch, 2003; Orrison et al., 2004; Harbring & Lünser, 2008; Lim et al., 2009; Müller & Schotter, 2010; Freeman & Gelber, 2010; Chen et al., 2011; Sheremeta, 2011; Shupp et al., 2013), or on the distribution of prizes across or within stages in dynamic elimination tournaments (Delfgaauw et al., 2012; Stracke et al., 2014). I contribute to this literature by studying the spread of prize money across periods in dynamic tournaments in conjunction with a positive weight of past performance.

In the rest of the study, I present the framework of Klein and Schmutzler (2014) and its results in a compressed form (Section 2.2), develop treatments and hypotheses (Section 2.3), describe the experimental design (Section 2.4), present the results (Section 2.5), and conclude (Section 2.6).

[4]This result holds for symmetric (or sufficiently symmetric) agents. Harbaugh and Ridlon (2010) show for an asymmetric all-pay auction that it is optimal to weigh past performance *negatively*.

2.2 A model of dynamic tournaments

2.2.1 Framework

The framework presented below captures the motivating example of an institution that repeatedly uses tournaments to induce efforts. The model corresponds to a particular parameterization of the normal-quadratic example of Klein and Schmutzler (2014), which itself is a special case of their general framework.[5] The specific parameterization allows me to derive closed-form solutions for the optimal choice of the policy parameters.

Suppose that there are two agents, $i \in \{1,2\}$, and two periods, $t \in \{1,2\}$. In each period, the agents choose effort levels $e_{it} \geq 0$ to maximize the difference between expected income and effort costs.[6] Effort costs are $K(e_{it}) = \frac{k}{2}(e_{it})^2$.

The principal's objective is to maximize total efforts, i.e. the participants' sum of first- and second-period efforts. At the end of each period t, the principal observes agent i's *performance* $s_{it} = e_{it} + \varepsilon_{it}$, which is an imperfect measure of the agent's effort e_{it}. ε_{it} is a stochastic observation error, which is independent across agents and periods. The principal has a fixed budget W for prizes. Specifically, the principal assigns the first-period prize $W_1 \in [0, W]$ to the agent with highest performance in period 1, so that agent i receives the prize if $s_{i1} > s_{j1}$.[7] Furthermore, the principal assigns what is left from the budget, $W_2 = W - W_1$, as a second-period prize to the agent with a highest weighted sum of performance in period 1 and period 2: Agent i receives W_2 if $s_{i2} + \eta s_{i1} > s_{j2} + \eta s_{j1}$. Here, $\eta \in \mathbb{R}$ is the weight of past performance in the second tournament. Note that $\eta > 0$ ($\eta < 0$) gives an advantage (disadvantage) to the agent with higher performance in period 1. If $\eta = 1$, W_2 goes to the agent with the highest sum of performance over both periods.

Hence, the principal sets two parameters in designing the optimal incentive system: (i) The spread of prize money across periods, which is uniquely determined by the first-period prize W_1, and (ii) the weight given to past performance in the second period, η. A particular policy is thus defined by the parameters η and W_1. Below, I first derive the behavior of the agents for given η and W_1. In the second step, I determine the optimal policy η^* and W_1^*.

[5] Specifically, the results follow from the normal-quadratic example of Klein and Schmutzler (2014) with $\sigma_1 = \sigma_2$.

[6] When using i and/or j as an index, I always assume $i, j \in \{1,2\}$ and $i \neq j$.

[7] In case of a tie, the principal assigns the prize with probability 0.5 to each agent.

2.2.2 Behavior of the agents

2.2.2.1 Second-period behavior

I start by analyzing the behavior of the agents in period 2 for given realizations of first-period performances s_{i1}.[8] The probability that agent i wins the second-period prize W_2 is $P\left(\eta s_{i1} + s_{i2} > \eta s_{j1} + s_{j2}\right)$. Let $\Delta s_{i1} = s_{i1} - s_{j1}$ be the *relative first-period performance* of agent i and $\Delta \varepsilon_{it} = \varepsilon_{i2} - \varepsilon_{j2}$ the *error difference* in period t. Note that $|\Delta s_{i1}| \neq 0$ implies that the agents' first-period performances were asymmetric, while $|\Delta s_{i1}|$ measures the size of this asymmetry. An agent with $\Delta s_{i1} > 0$ (< 0) had higher (lower) performance in the first period.

I assume that the distributions of the observation errors ε_{it} are such that their difference is normally distributed: $\Delta \varepsilon_{it} \sim \mathcal{N}\left(0, \sigma^2\right)$. In the following, I denote the density function of $\Delta \varepsilon_{it}$ by

$$\phi\left(s\right) = \frac{1}{\sigma\sqrt{2\pi}} \exp\left(-\frac{s^2}{2\sigma^2}\right),$$

and its cumulative density function by $\Phi\left(r\right) = \int_{-\infty}^{r} \phi\left(s\right) ds$. With the above notation and distributional assumptions, the probability that agent i wins the second-period prize can be written as $\Phi\left(\eta \Delta s_{i1} + e_{i2} - e_{j2}\right)$.

The expected second-period payoff of agent i, conditional on first-period performance and second-period effort choices, is thus

$$U_{i2}\left(e_{i2}, e_{j2}, \Delta s_{i1}\right) = \Phi\left(\eta \Delta s_{i1} + e_{i2} - e_{j2}\right) W_2 - \frac{k}{2} \cdot e_{i2}^2. \tag{2.1}$$

It depends on the realization of the relative first-period performance Δs_{i1}. Maximizing (2.1) for a given Δs_{i1} yields the first-order condition[9]

$$\phi(\eta \Delta s_{i1} + e_{i2} - e_{j2}) W_2 = k \cdot e_{i2}. \tag{2.2}$$

In the optimum, the agents equate marginal costs $k \cdot e_{i2}$ and marginal benefits of effort. The latter consist of the second-period prize W_2 weighted by the marginal increase of the probability of winning the prize $\phi(\eta \Delta s_{i1} + e_{i2} - e_{j2})$. Since ϕ is

[8] The derivations in this section resemble Aoyagi (2010) and Ederer (2010). While both are less restrictive with respect to the distributions of the observation error difference and the effort cost functions, they assume $W_1 = 0$ and $\eta = 1$.

[9] This treats a particular realization of Δs_{i1} as a subgame of first-period play. However, formally, there are no proper subgames in the second period. This is because the agents cannot distinguish which specific combination of first-period effort choices and first-period error realizations led to a certain Δs_{i1}, so that information sets are not singletons. Klein and Schmutzler (2014, pp. 10–11) show how the concept of Perfect Bayesian Equilibrium and an appropriate belief formation rule can be used to deal with this type of information structure. The solution of the game is nevertheless equivalent to that of a subgame-perfect Nash equilibrium.

symmetric, and because $(\eta\Delta s_{i1} + e_{i2} - e_{j2}) = -(\eta\Delta s_{j1} + e_{j2} - e_{i2})$, the left-hand side of (2.2) is identical for both agents, so that in equilibrium, the efforts of both agents must be equal.

Using $e_{i2} = e_{j2}$ in (2.2), I obtain the second-period equilibrium efforts:[10]

$$e_{i2}^{*}\left(\Delta s_{i1}; \eta, W_2\right) = \frac{\phi\left(\eta\Delta s_{i1}\right) W_2}{k} > 0. \tag{2.3}$$

Because $\phi > 0$, second-period efforts are always positive. The efforts react positively to the second-period prize W_2. Moreover, they are decreasing in the absolute value of the relative first-period performance Δs_{i1} and its weight η.[11] This means that with a non-zero weight of past performance, higher asymmetry in first-period performance decreases effort incentives in the second period. In turn, for given asymmetric first-period performance, a higher weight of past performance in absolute terms decreases second-period efforts. The reason is that with a nonzero weight of past performance, the principal essentially gives a headstart of $\eta\Delta s_{i1}$ in the second period to the agent with $s_{i1} > s_{j1}$, unless the realizations of first-period performance were exactly equal. This headstart reduces competition among the agents and decreases their effort incentives. Thus, a higher absolute value of first-period performance or of its weight, which both increase the headstart, reduces second-period efforts. Hence, for a nonzero weight of past performance to be optimal, it must have a positive effect on first-period efforts.

Finally, note that second-period efforts are symmetric. In particular, due to the symmetry of the distribution of the observation errors, second-period efforts are independent of the sign of the relative first-period performance Δs_{i1}. In other words, asymmetry in first-period performance decreases second-period efforts of both agents equally.

2.2.2.2 First-period behavior

In the first period, the agents take the effect of first-period effort on the tournament in the second period into account. I denote the expected second-period payoff, conditional on first-period efforts e_{i1} and e_{j1}, by $U_{i2}^{e}(e_{i1}, e_{j1})$. With $\Phi\left(e_{i1} - e_{j1}\right)$ as the probability of winning the first-period prize W_1, the expected first-period payoff of agent i, conditional on first-period effort choices, can be written as

$$\Phi\left(e_{i1} - e_{j1}\right) W_1 + U_{i2}^{e}\left(e_{i1}, e_{j1}\right) - \frac{k}{2} \cdot e_{i1}^{2}.$$

[10] In Appendix A2.2.1, I derive the second-order condition under which (2.2) characterizes a unique equilibrium in the second period.

[11] This follows from $\frac{\phi(a \cdot b)}{\partial a} > (<)\ 0 \Leftrightarrow a < (>)\ 0\ \forall\ a, b \neq 0$.

The corresponding first-order condition is

$$\phi\left(e_{i1}-e_{j1}\right)W_1+\frac{\partial U_{i2}^e}{\partial e_{i1}}=k\cdot e_{i1}. \tag{2.4}$$

In the first period, the agents equate marginal costs $k \cdot e_{i1}$ and marginal benefits of effort. The latter consist of the prize in the first period W_1 weighted by the marginal increase in the probability of winning $\phi\left(e_{i1}-e_{j1}\right)$, and the marginal effect of first-period effort on second-period payoff $\frac{\partial U_{i2}^e}{\partial e_{i1}}$. In Appendix A2.2.2, I show that for symmetric first-period effort choices, the latter can be written as

$$\left.\frac{\partial U_{i2}^e}{\partial e_{i1}}\right|_{e_{i1}=e_{j1}}=\frac{\eta}{\sigma\sqrt{2\pi}\sqrt{1+\eta^2}}W_2. \tag{2.5}$$

Using $\phi\left(0\right)=\frac{1}{\sigma\sqrt{2\pi}}$ and (2.5) in (2.4), I obtain the symmetric first-period equilibrium efforts as[12,13]

$$e_1^*\left(\eta,W_1,W_2\right)=\frac{1}{k\sigma\sqrt{2\pi}}\left(W_1+\frac{\eta}{\sqrt{1+\eta^2}}W_2\right). \tag{2.6}$$

We see that first-period efforts depend positively on the first-period prize W_1. The sign of the effect of the second-period prize W_2 depends on the sign of η: For positive (negative) weights, a higher second-period prize increases (decreases) incentives to exert effort in period 1 through the negative (positive) effect of more first-period effort on the second-period payoff.

Using $e_{i1}^*=e_{j1}^*$ in (2.3) and taking the expectation over the error difference in period 1 yields the expected value of the second-period efforts (see Appendix A2.2.4):

$$E\left(e_2^*\left(\eta,W_2\right)\right)=\frac{W_2}{k\sigma\sqrt{2\pi}\sqrt{1+\eta^2}}. \tag{2.7}$$

Note that second-period efforts are decreasing in the size of the weight η. The reason is analogous to the discussion in Section 2.2.2.1: The expected asymmetry in the second period increases with the absolute value of the weight of past performance, which decreases (expected) second-period effort incentives.

[12]In Appendix A2.2.3, I derive the second-order condition under which (2.6) characterizes a unique symmetric equilibrium in the first period.

[13]Note that I neglect the potential existence of asymmetric equilibria in the first period. In the second period, however, there only exists a symmetric equilibrium, which is unique.

2.2.3 Optimal policy

Since first-period efforts are deterministic and efforts are symmetric across agents, the principal's payoff is $V(\eta, W_1) = e_1^*(\eta, W_1, W - W_1) + E(e_2^*(\eta, W - W_1,))$.[14] With (2.6) and (2.7), it can be written as

$$V(\eta, W_1) = \frac{1}{k\sigma\sqrt{2\pi}} \left(W_1 + \frac{1+\eta}{\sqrt{1+\eta^2}} (W - W_1) \right).$$

I first focus on the optimal choice of the weight of past performance, η. Note that the partial derivative

$$\frac{\partial V(\eta, W_1)}{\partial \eta} = \frac{W - W_1}{k\sigma\sqrt{2\pi}} \frac{1-\eta}{(1+\eta^2)^{\frac{3}{2}}}$$

is positive for all $\eta < 1$, 0 for $\eta = 1$, and negative for all $\eta > 1$, given that $W_1 < W$. To understand this, suppose the principal starts with a weight of 0 and increases it gradually to 1. From (2.6) and (2.7), we know that this gives additional incentives for first-period efforts, but reduces second-period efforts through the introduction of asymmetry between the participants in period 2. What the result on $\frac{\partial V(\eta, W_1)}{\partial \eta}$ tells us is that as long as η is smaller than 1, the positive effect of increasing the weight on first-period efforts dominates the detrimental effect of limiting competition in the second period. The effects cancel out for a weight of 1. When increasing the weight even further, the detrimental effect on second-period efforts exceeds the positive effect on first-period efforts. This implies:

Corollary 2.1. *For all tournaments with a positive second-period prize W_2 (if $W_1 < W$), the optimal weight of past performance is $\eta^* = 1$.*

This means that it is always optimal to weigh past performance just as strongly as current performance for the second-period prize, given that there is a second-period prize at all.

I now focus on the optimal spread of prize money across the two periods. I obtain

$$\frac{\partial V(\eta, W_1)}{\partial W_1} = \frac{1}{k\sigma\sqrt{2\pi}} \left(1 - \frac{1+\eta}{\sqrt{1+\eta^2}} \right).$$

This expression is negative for all $\eta > 0$. This means that if the principal weighs first-period performance positively in the assignment of the second-period prize, second-period prize money is more effective in incentivizing efforts than first-period prize money. Indeed, with a positive η, second-period prize money affects both first- and second-period efforts, while first-period prize money is only relevant to first-period efforts. I obtain:

[14] Note that from now on, I impose the principal's budget constraint $W_2 = W - W_1$.

Corollary 2.2. *For all tournaments with a positive weight of past performance (if $\eta > 0$), the optimal first-period prize is $W_1^* = 0$.*

Therefore, if the principal sets a positive weight of past performance, all prize money should be paid out in the second period.

I now combine Corollary 2.1 and Corollary 2.2:

Corollary 2.3. *The optimal policy is $\eta^* = 1$ and $W_1^* = 0$.*

Hence, if the principal controls both η and W_1, it is optimal to set $\eta = 1$ and $W_1 = 0$. To see this, note that Corollary 2.1 and Corollary 2.2 already imply that $\eta = 1$ and $W_1 = 0$ is the optimal policy among those with $W_1 < W$. The resulting payoff is

$$V(1,0) = \frac{1}{k\sigma\sqrt{2\pi}}\sqrt{2}W.$$

For any other policy with $W_1 = W$, the principal's payoff is

$$V(\eta, W) = \frac{1}{k\sigma\sqrt{2\pi}}W.$$

Obviously, $V(1,0) > V(\eta, W)$, so that $\eta = 1$ and $W_1 = 0$ is indeed globally optimal.

2.3 Treatments and hypotheses

With the results from the preceding section, I can now develop normative prescriptions for a principal who, in every period, uses independent tournaments to induce efforts. Again, I restrict myself to two periods. Suppose that in both periods, the prize which the principal assigns to the best-performing agent is $\frac{W}{2}$. In terms of the framework introduced above, these incentives correspond to a policy with weight $\eta = 0$, first-period prize $W_1 = \frac{W}{2}$, and budget W. I call this the *baseline policy* and denote the treatment that implements the baseline policy by *BASE*. Corollary 2.1 and Corollary 2.2 inform the principal on how to improve upon the baseline policy.

Corollary 2.1 tells us that for a given prize structure with equal prizes of $\frac{W}{2}$ in each period, the optimal choice of the weight of past performance is $\eta = 1$. This means that instead of assigning a second-period prize of $\frac{W}{2}$ to the agent who performs best in period 2, the principal should assign the second-period prize to the agent who has highest sum of performance over both periods. I call the policy with weight $\eta = 0$ and first-period prize $W_1 = \frac{W}{2}$ the *partially optimal policy* and denote the treatment implementing the partially optimal policy by *PART*. This yields:

Hypothesis 2.1. *Expected total efforts in PART are higher than expected total efforts in BASE.*

Table 2.1: Policies

	η	W_1	W_2
BASE	0	$\frac{W}{2}$	$\frac{W}{2}$
PART	1	$\frac{W}{2}$	$\frac{W}{2}$
GLOB	1	0	W

Table 2.2: Point predictions of efforts

	BASE	PART	GLOB
Total effort	45.4	54.7	64.1
Effort 1	22.7	38.7	32.1
Effort 2	22.7	16.0	32.1

According to Corollary 2.2, effort provision can be further improved, since for a positive weight of past performance, the optimal choice of the prizes is $W_1 = 0$ and $W_2 = W$. Hence, there should not be any prize for the best performing agent in period 1. Instead, the principal should assign all prize money to the agent with highest sum of performance over both periods. Corollary 2.3 implies that this choice of policy parameters is the optimal policy. I thus call the policy with weight $\eta = 1$ and first-period prize $W_1 = 0$ the *globally optimal policy* and denote the treatment implementing the globally optimal policy by *GLOB*. I obtain the following two hypotheses:

Hypothesis 2.2. *Expected total efforts in GLOB are higher than expected total efforts in PART.*

Hypothesis 2.3. *Expected total efforts in GLOB are higher than expected total efforts in BASE.*

See Table 2.1 for an overview of the three policies.

To gain an understanding of the size of the effects, I use the following calibration for the model parameters: $k = 0.066$, $\sigma = 40$, and $W = 300$.[15] Table 2.2 contains the resulting point predictions for expected total effort, as well as the point predictions for the (expected) efforts in period 1 and period 2.

We see that from a theoretical standpoint, choosing the policy parameters optimally has substantial effects on effort provision (see row "Total effort" of Table 2.2), while it is budget-neutral to the principal. The optimal adjustment of the weight

[15]These parameter values satisfy the second-order conditions in the first (A2.3) and second period (A2.1).

from BASE to PART (the *weight adjustment*) increases expected total efforts by more than 20%. Further adjusting the prizes optimally from PART to GLOB (the *prize adjustment*, which is conditional on the optimal adjustment of the weight) increases expected total efforts by another 17%. Overall, jointly adjusting both the weight and the prizes optimally from BASE to GLOB (the *weight-prize adjustment*) increases expected total efforts by roughly 41%. Note that both in PART and in GLOB the principal does not spend more money on prizes than in BASE.

The point predictions also illustrate how the policy adjustments affect the distribution of efforts across periods (see rows "Effort 1" and "Effort 2" of Table 2.2). The weight adjustment increases first- and decreases (expected) second-period efforts. As discussed above, weighting past performance positively in the second period gives an incentive to exert more effort in the first period. However, the positive weight of past performance induces asymmetry in the second period, which reduces the incentives to exert effort in period 2. Furthermore, shifting all prize money to the second period as through the prize adjustment decreases first-period efforts and increase second-period efforts again. Altogether, the weight-prize adjustment increases both first- and second-period efforts.

2.4 Experimental design

2.4.1 Laboratory experiment

I conducted three experimental sessions, each consisting of 30 rounds. Every treatment was repeated for 10 rounds. The order of the treatments varied across sessions (see Table 2.3). To avoid order effects, the goal in this variation was to alter, across sessions, the predecessor of each treatment. The variation of the positions of the treatments was only second priority.[16]

At the beginning of the sessions, the participants were randomly assigned to matching groups of size 8, and, in every round, formed randomly determined pairs with another participant from their matching group.[17] Before the first treatment of a session, the participants received instructions about the general structure of the experiment and instructions specific to the first treatment. Only after the end of the first (second) treatment, they received the instructions specific to the second (third) treatment.[18] The instructions were always read out aloud after they had

[16]Therefore, the design is not perfectly balanced. With my order of the treatments, every treatment is preceded once by every other treatment, except for BASE, which is preceded twice by PART. With a perfectly balanced design, every treatment would have been preceded twice by the same treatment.

[17]The participants were aware that in every round, the other participant in their pair was randomly chosen. The instructions did not, however, mention the existence of matching groups.

[18]Appendix A2.6 contains the instructions.

Table 2.3: Order of treatments

	Session 1	Session 2	Session 3
Rounds 1 – 10	BASE	PART	GLOB
Rounds 11 – 20	PART	BASE	PART
Rounds 21 – 30	GLOB	GLOB	BASE

been distributed. Before the start of the experiment, the participants had to solve quizzes on the computer screen, which contained control questions on the general structure of the experiment. Before starting with a treatment, they also had to answer control questions specific to this treatment.[19]

Each round consisted of two periods. In the first period, the participants in a pair simultaneously chose effort levels between 0 and 55 in increments of 0.5 (see screenshot on page 111). The instructions referred to effort as "input" and contained a table and a graph depicting the costs the participants would have to pay at the end of each round for choosing any of the admitted input levels (see instructions on page 107). The underlying cost function was $K\left(e_{it}\right) = 0.033 \cdot e_{it}^2$.[20]

After both participants in a pair had chosen an effort level, the computer drew random numbers for both participants from a normal distribution with expected value of zero and a standard deviation of 28.28.[21] The instructions illustrated the symmetry of the distribution and that small random numbers were more likely than large numbers. Furthermore, the instructions contained a graph depicting the distribution of the random number and an explanation of how to read the graph (see instructions on page 108). The instructions also emphasized that the draws of the random numbers were independent over time and between participants.

In the next step, the computer determined the participants' performance (referred to as "output") as the sum of their effort level and the random number. The instructions emphasized that on average, their output level would be equal to their input level, and explained in which way the output level would deviate from the input level through its dependence on the random number. At the end of the first period, the participants saw their own performance, the performance of the other participant in their pair, and their relative performance Δs_{i1} (see screenshot on page 112) on the computer screen.

Period 2 worked in the same way as period 1: At the beginning, the participants simultaneously chose effort levels. Then, the computer drew random numbers, calculated the performance levels, and displayed the participants' own and their paired participant's performance level as well as their relative performance Δs_{i2}.

[19] Appendix A2.7 contains the control questions.
[20] This implies $k = 0.066$.
[21] $\varepsilon_{it} \sim \mathcal{N}\left(0, 28.28^2\right)$ implies $\Delta\varepsilon_{it} \sim \mathcal{N}\left(0, 40^2\right)$

After the second period, the computer distributed a total of 300 points as prizes within each pair based on the participants' performance levels in both periods. The instructions referred to the prizes as "payments". The assignment rules in the three treatments were the following:[22]

- BASE $\begin{cases} \text{150 points to participant with higher performance in period 1} \\ \text{150 points to participant with higher performance in period 2} \end{cases}$

- PART $\begin{cases} \text{150 points to participant with higher performance in period 1} \\ \text{150 points to participant with higher sum of performance in period 1 and period 2} \end{cases}$

- GLOB $\begin{cases} \text{300 points to participant with higher sum of performance in period 1 and period 2} \end{cases}$

The participants' payoff from a particular round was then equal to the sum of the prizes they had received in this round and an endowment of 200 points, less the costs for their effort levels in both periods of this round. The endowment ensured that a participant's payoff from a round would never be below 0. At the end of each round, the participants saw their payments, their costs for effort, and the their resulting payoff on the computer screen (see screenshot on page 113).

At the end of the experiment, the computer randomly chose one of the 30 rounds as the one to be paid out to all of the participants (see Cubitt et al., 1998, for the validity of this approach). Afterwards, the participants were informed about the round that was chosen for payoff, and were able to review all information they had received during the 30 rounds.

2.4.2 Pre-experimental questionnaire

I used a pre-experimental questionnaire to elicit individual attributes that may explain variation in effort choices, and ideally in treatment effects, across participants. Note that a common observation in experiments on tournaments (rank-order tournaments, lottery contests and all-pay auctions) is the large heterogeneity in behavior across participants. According to Dechenaux et al. (2012), important factors explaining parts of this variation are individual heterogeneity in (1) social preferences, (2) risk aversion, (3) loss aversion and (4) non-monetary preferences towards winning.[23] The pre-experimental questionnaire contained measures to address each of these factors.

[22] In case that the performances or the sum of performances was equal, the computer drew the receiver of the corresponding prize randomly from both participants.

[23] Dechenaux et al. (2012) also mention demographic differences as relevant factors. In my data, sex and age of the participants have no explanatory power.

To elicit the magnitude of social preferences, I used the SVO Slider Measure developed by Murphy et al. (2011). It allows me to categorize the participants into four classical social value orientation types based on their choices in a sequence of six dictator games (see Table A2.1). The orientation types are the following: *Altruists*, who maximize the payoff of the other, *prosocials*, who maximize the joint payoff of themselves and the other (without distinction between inequality averse and inequality tolerant subjects), *individualists*, who maximize their own payoff, and *competitors*, who maximize the difference between their own and the other's payoff.[24] Murphy et al. (2011) show that the SVO Slider Measure is a reliable and valid method to elicit social value orientation. For further details, see Appendix A2.3.1.

To assess the participants' risk aversion, I used a lottery task similar to the one used by Dohmen et al. (2011). In the task, the participants had to make decisions for six choice situations, each between a positive safe payoff and a lottery. The lottery was the same across situations, while the value of the safe payoff varied between situations from above to below of the expected value of the lottery. When ordering the situations in decreasing size of the safe payoff (see Table A2.3), the point at which the participants switch from choosing the safe payoff to choosing the lottery provides a measure for their degree of risk aversion. I denote this switching point by *SPRISK*. Appendix A2.3.2 contains further details.

To measure loss aversion in risky choices, I employed the lottery task developed by Gächter et al. (2010). In this task, the participants have to accept or reject six lotteries. The lotteries yield, with equal chances, a positive payoff and a negative payoff. While the positive payoff is constant, the absolute value of the negative payoff varies across lotteries between above to below of the value of the positive payoff. When the lotteries are ordered in increasing size of the negative payoff (see Table A2.4), the point at which the participants start rejecting the lotteries is informative about their degree of loss aversion. More specifically, the switching point yields the measure λ, which is the coefficient of loss aversion in a simple linear utility approximation around 0. See Appendix A2.3.3 for further details.

In order to address the issue of heterogeneity in preferences towards winning, I used the Revised Competitiveness Index developed by Houston et al. (2002), which aims at measuring "a desire to win in interpersonal situations" (p. 31). The index is reliable (Harris & Houston, 2010) and correlates positively with other indices of competitiveness (Houston et al., 2002). It consists of several questions about competition in daily life contexts (see Table A2.6). The participants' answers to the questions yield the overall index value *RCI*. Appendix A2.3.4 contains further details.

Table 2.4 provides an overview of the measures generated in the questionnaire.

[24]Murphy et al. (2011) also propose a method which is able to distinguish inequality averse prosocials from inequality tolerant prosocials, consisting of 9 additional dictator games.

Table 2.4: Measures from pre-experimental questionnaire

Variable	Measure for	Range of values
SVO	social value orientation	altruist, prosocial, individualist, competitive
SPRISK	risk aversion	1, 2, ..., 7
λ	loss aversion	0.87, 1, 1.2, 1.5, 2, 3
RCI	preference towards winning	14, 15, ..., 70

Previous research allows me to form hypotheses on the signs of the relationships between the measures and effort levels. For instance, several papers document that more risk-averse participants exert lower efforts in tournaments (Millner & Pratt, 1991; Anderson & Freeborn, 2010; Sheremeta & Zhang, 2010; Price & Sheremeta, 2011, 2012; Sheremeta, 2011; Sheremeta et al., 2013). Hence, I expect the relation between SPRISK and effort to be negative. Similarly, the literature usually associates loss aversion with lower efforts (Kong, 2008; Jönsson, 2013; Shupp et al., 2013), implying a negative relationship between λ and effort. Furthermore, a higher preference towards winning seems to increase efforts (Sheremeta, 2010b; Price & Sheremeta, 2011, 2012; Sheremeta et al., 2013; Brookins & Ryvkin, 2014), which suggests that RCI relates positively to effort.

In contrast to the measures for risk aversion, loss aversion and preference towards winning, I have no hypothesis on the relationship between the different social value orientation types and efforts. This is because the SVO Slider Measure is difficult to compare to the social preference measures in previous experiments on tournaments. For example, Balafoutas et al. (2012) characterize participants according to their acceptance of advantageous and disadvantageous inequality. It is not clear how a participant's choices in this specific elicitation method would relate to choices in the SVO Slider Measure.[25]

2.4.3 Procedural details

A total of 96 subjects participated in the experiment. The sessions had 32 participants each, lasted for about 120 minutes and took place at the computer lab of the University of Zurich, Switzerland, in early December 2013. All participants completed the pre-experimental questionnaire at least eight days ahead of the labo-

[25]Using dictator games rather similar to those in the SVO Slider Measure, Hernandez et al. (2013) distinguish between selfish and other-regarding subjects and show that the former exert more effort than the latter. For my experiment, this would imply that individualists exert more effort than prosocial participants. Nevertheless, this does not imply anything for participants characterized as competitive by the SVO Slider Measure.

ratory experiment.[26] The participants were recruited from local university students, excluding economics and psychology majors.[27] I used z-Tree (Fischbacher, 2007) to program and conduct the experiment and the on-line tool Qualtrics[28] to administer the pre-experimental questionnaire. The design of the experiment and the questionnaire guaranteed complete anonymity of a participant's decisions and payoffs to other participants.

Immediately after the laboratory experiment, the participants received, individually and in private, the sum of their payoffs from the laboratory experiment and the pre-experimental questionnaire plus a participation fee of 10 Swiss Francs (CHF).[29,30] The average total payoff was CHF 50.35, consisting of an average of CHF 36.28 from the laboratory experiment and an average of CHF 14.07 from the pre-experimental questionnaire. In the laboratory experiment, the exchange rate to convert points to CHF was 10 points per CHF.[31]

2.5 Results

I split the analysis of the experimental data into three parts. In the first part, I test the predictions of the theoretical model, while the second part concerns the explanation of individual heterogeneity in behavior. The remaining part focuses on the effect of first-period asymmetry on second-period efforts.

[26] The average processing time for the questionnaire was about 13 minutes.

[27] The recruitment was conducted with the software hroot (Bock et al., 2012).

[28] See http://www.qualtrics.com.

[29] Because of the delay of payments from the pre-experimental questionnaire, the instructions for the questionnaire stressed that the decisions of the participants would have real monetary consequences.

[30] To match the payoffs from the questionnaire to the payoffs from the laboratory experiment, I used the names of the participants, which they provided in both parts.

[31] At the time of the experiment, the exchange rate was CHF 1.23 per € and CHF 0.91 per US$.

2.5.1 Predictive power of the theoretical model

Figure 2.1 shows the policy effects on the means of total as well as first- and second-period efforts.[32,33]

As the central result, I obtain:

Result 2.1. *The weight-prize adjustment increases total efforts. The increase is about 10% compared to the baseline policy. There is no evidence for an effect of the weight adjustment on total efforts. The prize adjustment (conditional on the weight adjustment) increases total efforts by about 9% compared to the partially optimal policy.*

Evidence for Result 2.1. Row “Total Effort” of Table 2.5 documents the result. It contains the sign predictions for the effects of the policy adjustments, as well as p-values from tests of one-sided hypotheses that the observed total efforts change as predicted. Note that the observations are clustered within matching groups.[34] Therefore, these and all following hypothesis tests rely on the signed-rank test proposed by Datta and Satten (2008), which provides high power while maintaining the correct size even if the observations are clustered. The unit of observation is always the mean of a participant’s (total or period-specific) effort across all 10 rounds of a policy. Thus motivated, we see that total efforts do not increase significantly[35] through the weight adjustment ($p = 0.856$). However, total efforts increase significantly through the weight-prize adjustment ($p = 0.006$). Furthermore, total efforts increase significantly through the prize adjustment ($p = 0.001$) (conditional on the

[32]Figure A2.1 depicts the mean first- and second-period effort choices over the 10 rounds of each policy. It reveals a slight downward trend in first- and second-period efforts under the baseline policy. Under the partially and the globally optimal policy, there is a short-lived upward trend in first-period efforts, which fades away after the first four rounds. The downward trend in the baseline policy may be due to a learning effect. However, note that the baseline policy was the first policy in a session just as often as the partially and the globally optimal policy, and that these policies do not exhibit a similar time trend. Hence, it seems unlikely that learning is the reason for the time trend under the baseline policy. Furthermore, Figure A2.1 shows that second-period efforts exhibit a higher variation over time than first-period efforts under all three policies. For the partially and the globally optimal policy, this is in line with the theoretical model, as in both of these policies, the second-period behavior depends on first-period observation noise.

[33]Figure A2.2 contains histograms of first- and second-period efforts under all three policies. Note that there are spikes at 0 and 55 (the left and right margin of the choice set), as well as at 30. Apparently, 30 is a focal point in the choice set. Since focal points are most likely independent of the policies, the existence of focal points should not conflict with my identification strategy.

[34]This means that observations are dependent within matching groups. The dependence within matching groups arises through the repeated interaction of the participants with each other over the course of the experiment. Since participants interacted only with other participants from the same matching group, observations are independent across matching groups.

[35]In this study, I call $p < 0.05$ significant and $p < 0.10$ marginally significant.

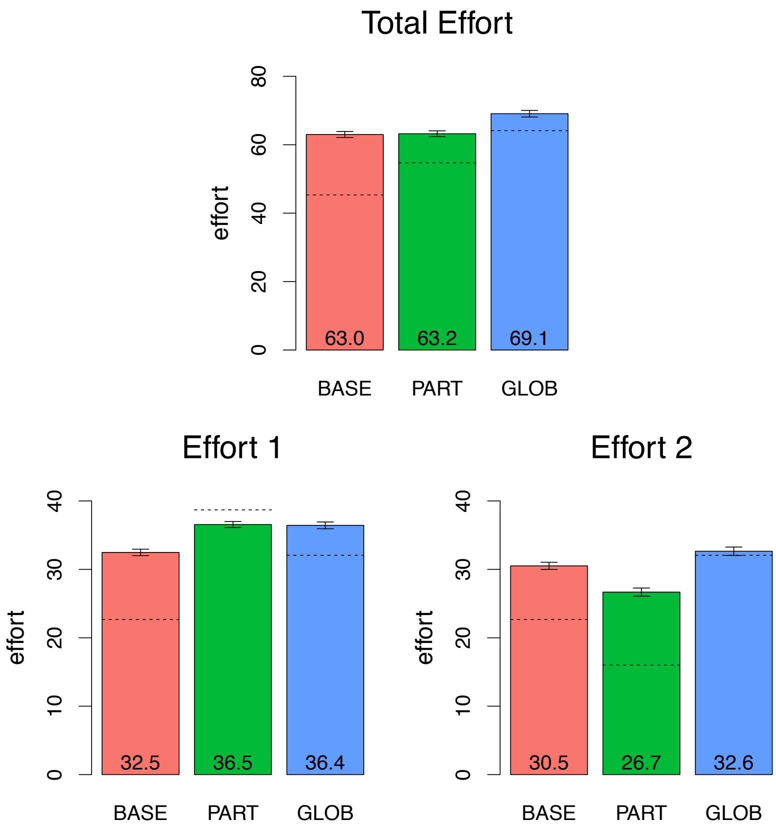


Figure 2.1: Means of efforts under the three policies. Heights of bars and values at bottom of bars correspond to means of efforts. Lengths of whiskers at top of bars are equal to standard errors of the means. Dotted lines depict Nash predictions for the corresponding policy. $N = 960$ per policy. Sample: All participants.

weight adjustment).[36] The result is robust to different testing procedures.[37] The percentage increases of total efforts through the weight-prize adjustment and the

[36]Note that the policies came in different orders across sessions. I find that when GLOB comes first (session 3), the effect of the weight-prize adjustment from BASE to GLOB is significantly higher than when BASE came first or second and GLOB third (sessions 1 and 2). This may be due to spillovers from BASE and PART to GLOB in sessions 1 and 2. However, to investigate such order effects further, it would be necessary to run additional sessions. In my data, I cannot distinguish order effects from session-fixed effects, as I only observe one session per order. In any case, note that the effect of the prize adjustment does not differ significantly between sessions, suggesting that there are no order effects for the prize adjustment. Similarly, the effect of the weight adjustment (which I find to be zero) does not differ significantly between sessions.

[37]When using Wilcoxon signed-rank tests based on matching-group means, which rely on less observations than the test by Datta and Satten (2008), I obtain the same result. Furthermore, Tobit estimations of the policy adjustment effects, which impose more distributional assumptions than the non-parametric tests but take the censoring of effort choices into account, also yield the same result.

prize adjustment follow from the means of total efforts under BASE (63.0), PART (63.2) and GLOB (69.1) shown in Figure 2.1.[38]

Table 2.5: Tests of effects of policy adjustments

	Weight adj.		Prize adj.		Weight-prize adj.	
	Pred.	P-val.	Pred.	P-val.	Pred.	P-val.
Total effort	+	0.856	+	0.001	+	0.006
Effort 1	+	0.015	−	0.721	+	0.005
Effort 2	−	0.001	+	0.001	+	0.076

Sign predictions derived from comparison of Nash level predictions for the different policies. Signed-rank tests according to Datta and Satten (2008) based on each participant's mean efforts under the corresponding policies ($N = 96$). One-sided hypotheses, with H_1 corresponding to the prediction. Sample: All participants.

This means that in order to increase total efforts compared to the baseline situation, it is necessary to adjust both the weight and the prizes optimally. However, the effect is only a fourth of the predicted increase (10% vs. 41% through the weight-prize adjustment). While the weight adjustment does not increase total efforts, it still has an effect on behavior. In fact, I obtain:

Result 2.2. *(i) All three policy adjustments affect the behavior of the participants and change the distribution of efforts across periods. (ii) Moreover, the predictions for the directions of the effects on first- and second-period efforts are correct for the weight adjustment and for the weight-prize adjustment. For the prize adjustment, the prediction for the direction of the effect is only correct in the second period.*

Evidence for Result 2.2. I start with part (ii), which follows from rows "Effort 1" and "Effort 2" in Table 2.5. In line with the predictions of the theoretical model, the weight adjustment increases first-period efforts ($p = 0.015$) and decreases second-period efforts ($p = 0.001$). Furthermore, the weight-prize adjustment increases both first-period efforts ($p = 0.005$) and second-period efforts ($p = 0.076$) at least marginally significantly, which is again in line with the prediction. The effect of the prize adjustment is not consistent with the prediction in that it does not decrease first-period efforts significantly ($p = 0.721$). However, again in line with the pre-

[38]The censoring of total effort may potentially lead to an underestimation of the increase when comparing the means of total efforts between policies. However, Tobit estimations of the policy adjustment effects, which correct for the censoring bias, yield an only marginally higher effect of the weight-prize adjustment and of the prize adjustment.

diction, it increases second-period efforts significantly ($p = 0.001$).[39] The result is robust to using different testing procedures.[40] Part (i) follows from part (ii).

Result 2.2 (ii) implies that the theoretical model performs fairly well in capturing the directions of the incentive effects of the policy parameters.[41] Given Result 2.1, this means that it mainly fails in the prediction of the size of the effects. This holds particularly for the weight adjustment, for which the increase of first-period efforts does not, unlike predicted, compensate the decrease of second-period efforts. As a consequence, the weight adjustment does not increase total efforts.

Focusing on the accuracy of the level predictions, I obtain:

Result 2.3. *(i) There is significant over-expenditure of total effort under all three policies. (ii) The extent of over-expenditure decreases with the stepwise adjustment of the policy parameters towards the optimum.*

Evidence for Result 2.3. Row "Total effort" of Table 2.6 implies the result. It contains the point predictions for total efforts under the three policies, as well as the corresponding observed means. Furthermore, it contains p-values of two-sided hypotheses that the efforts are equal to the predictions. We see that total efforts are significantly higher than the prediction in BASE ($p = 0.002$), PART ($p = 0.010$) and GLOB ($p = 0.050$)[42] , which implies part (i).[43,44] When comparing the predictions

[39]I find that the effect of the weight adjustment on first- and second-period efforts does not differ significantly between sessions, suggesting that there are no order effects for the weight adjustment. The same holds for the effect of the weight-prize adjustment on first-period efforts. However, the effect of the weight-prize adjustment on second-period efforts differs between sessions in a way that is similar to the finding for total efforts: When GLOB comes first (session 3), the effect of the weight-prize adjustment on second-period efforts is significantly higher than when PART comes first or second and GLOB third (sessions 1 and 2). Furthermore, the effect of the prize adjustment on first- and on second-period efforts seems to vary at least marginally significantly between the sessions in a way that is difficult to interpret. As argued above, for further investigation, it would be necessary to collect more data.

[40]I obtain the same result when using Wilcoxon signed-rank tests based on matching group means or a Tobit model.

[41]The result that the introduction of a positive weight of past performance increases first- and decreases second-period efforts is consistent with experimental results on lottery contests in which the introduction of a carryover of first-period efforts into the second period has the same effect (Schmitt et al., 2004; Sheremeta, 2010a).

[42]More precisely, $p = 0.04997758$.

[43]I find that under all policies, over-expenditure in total effort is always significantly higher in session 2 than in session 1, while over-expenditure in session 3 always lies between over-expenditure in sessions 1 and 2. This may be due to order effects. However, since I observe the same ranking of over-expenditure between sessions for all three policies, this observation may as well be due to session-fixed effects. To systematically relate over-expenditure to order, it would be necessary to collect more data.

[44]Tobit regressions of over-expenditure on a constant yield the same result. When using Wilcoxon signed-rank tests based on matching group means, the result changes insofar as I cannot reject no over-expenditure in GLOB ($p = 0.110$).

with the observed means, we see that in BASE, over-expenditure is about 17.7 units, which amounts to 40% of the predicted level of total effort. In PART, over-expenditure is only 8.5 units or 16%, in GLOB 5 units or 8%.[45] This implies part (ii).

Table 2.6: Tests of level predictions

	BASE			PART			GLOB		
	Pred.	Mean	P-val.	Pred.	Mean	P-val.	Pred.	Mean	P-val.
Total effort	45.3	63.0	0.002	54.7	63.2	0.010	64.1	69.1	0.050
Effort 1	22.7	32.5	0.002	38.7	36.5	0.381	32.1	36.4	0.015
Effort 2	22.7	30.5	0.002	16.0	26.7	0.001	32.1	32.6	0.507
Eff. 2 $\|\Delta s_{i1}$	–	–	–	15.2	26.7	0.001	30.9	32.6	0.117

Signed-rank tests according to Datta and Satten (2008) based on each participant's mean effort under the corresponding policy ($N = 96$). Two-sided hypotheses. Sample: All participants.

Result 2.3 implies that one reason for the failure of the theory in predicting the size of the policy adjustment effects is that it substantially underpredicts efforts in the baseline policy compared to the other two policies.

The presence of over-expenditure contrasts with observations from most other experiments on rank-order tournaments: As Dechenaux et al. (2012) point out in their survey article on contest experiments, over-expenditure of efforts is quite common for lottery contests and all-pay auctions, but an exception in rank-order tournaments. The authors argue that potential reasons for this absence of over-expenditure are the convexity of costs and the presence of noise in the decision problem of the agents, since both are unique features of rank-order tournaments in comparison to the two other forms of contests. However, these reasons cannot explain the difference in over-expenditure between policies in this experiment, as costs and noise structure are constant across all three situations.

To analyze the issue of over-expenditure further, I now focus on the accuracy of the period-specific effort predictions. Note that for the second period and a positive weight of past performance (as under the partially and the globally optimal policy), the theoretical model provides two types of level predictions: (1) A prediction of the average value of second-period efforts across all realized tournaments, and (2) a prediction of second-period efforts conditional on relative first-period performance Δs_{i1}. I obtain:

[45] Note that one reason for the decrease of over-expenditure with the adjustment of the policy parameters towards the optimum may be that the predictions come closer to the upper limit of the choice set (110). However, when estimating the amount of over-expenditure with a Tobit model, which corrects for this censoring bias, I obtain, under all three policies, only marginally higher over-expenditure.

Result 2.4. *(i) Under the baseline policy, over-expenditure occurs in both periods. Under the partially (the globally) optimal policy, over-expenditure occurs only in the second (only in the first) period. (ii) The results on the second period under the partially and the globally optimal policy do not change when considering predictions for second-period efforts conditional on relative first-period performance.*

Evidence for Result 2.4. Part (i) draws support from row "Effort 1" and row "Effort 2" of Table 2.6. In BASE, first- and second-period efforts are significantly higher than the prediction ($p = 0.002$ and $p = 0.002$). In PART, first-period efforts do not differ significantly from the prediction ($p = 0.381$), while second-period efforts are significantly higher than the prediction ($p = 0.001$). For GLOB, the result is reversed: First-period efforts are significantly higher than the prediction ($p = 0.015$), while second-period efforts do not differ significantly from the prediction ($p = 0.507$).[46] This is robust to different testing procedures.[47] Part (ii) follows from row "Eff. 2 $|\Delta s_{i1}$" of Table 2.6. The values for the predictions in this row result from calculating, for every observed Δs_{i1}, the conditional prediction of second-period efforts as implied by (2.3), and calculating the mean of the conditional predictions over all observed Δs_{i1}. The p-values in this row result from signed-rank tests based on a participant's mean deviation in second-period effort choices from the corresponding conditional predictions, using two-sided hypotheses that the deviations are 0. As in the case of the unconditional predictions, second-period efforts are significantly higher than the conditional predictions in PART ($p = 0.001$) and not significantly different from the conditional predictions in GLOB ($p = 0.117$).[48]

In summary, this part demonstrates that the predictive power of the theoretical model is mixed. In Result 2.1, I show that the weight-prize adjustment increases total efforts, although to a smaller extent than predicted (10% vs. 41%). Furthermore, I show that total efforts do not increase with the weight adjustment. In Result 2.2, I demonstrate that all policy adjustments change the distribution of efforts across periods, while the directions of these effects correspond widely to the predictions of the theoretical model. According to Result 2.3, while I observe significant over-expenditure of total effort under all three policies, over-expenditure decreases with

[46]For over-expenditure in first- and second-period efforts under BASE, I find the same result as for over-expenditure in total effort: Over-expenditure is always significantly higher in session 2 than in session 1, while over-expenditure in session 3 always lies between over-expenditure in sessions 1 and 2. Under PART, I find that in contrast to the overall result, there is significant over-expenditure in first-period effort in sessions 2 and 3. Over-expenditure in second-period effort under PART does not differ between sessions. Under GLOB, over-expenditure in first-period effort does not differ between sessions. However, in contrast to the overall result, I find significant over-expenditure in second-period efforts in sessions 2 and 3. All these results may be due to ordering or session-fixed effects.

[47]The result does not change when using Wilcoxon signed-rank tests based on matching group means or a Tobit model.

[48]Part (ii) does not change when using Wilcoxon signed-rank tests based on matching group means.

the weight adjustment and with the prize adjustment. Finally, Result 2.4 focuses on the accuracy of period-specific level predictions.

2.5.2 Explaining heterogeneity

So far, I have focused on average behavior. Note that the theoretical model, which relies on symmetric agents, predicts effort choices to be equal within pairs. This holds for all three policies, and, in particular, both for the first and for the second period.[49] However, the behavior of the participants is in stark contrast to this prediction:

Result 2.5. *There is a substantial degree of heterogeneity in behavior across individuals under all three policies.*

Evidence for Result 2.5. The result follows from the histograms in Figure 2.2, which show, for both periods and all three policies, the observed variation in the size of the effort difference between both members of a pair. The prediction of equal efforts corresponds to an effort difference of 0. Although there is a considerable share of pairs whose effort difference is indeed close to 0, there are much more pairs with an effort difference that is positive, implying asymmetric effort choices of both members of the pair. In fact, the means of first- and second-period effort differences under the three policies lie between 14.5 and 20.6 units.

In the following, I analyze explanations for the observed heterogeneity across participants. To this end, I relate the measures obtained in the pre-experimental questionnaire to individual behavior in the laboratory.

Figure 2.3 contains histograms of the measures.[50] We see that the elicitation of social value orientation identifies only one participant as an altruist, while no participant falls into the competitive category. In fact, consistent with the findings of Murphy et al. (2011), the majority of the participants (60 out of 95) are prosocials. In the following analysis, I therefore only distinguish between prosocial and non-prosocial participants by using the dummy variable PROSOC as an indicator for prosociality. Furthermore, we see that the majority of the participants is risk-averse (SPRISK > 3, see Appendix A2.3.2) and loss averse ($\lambda > 1$, see Appendix A2.3.3). Finally, the mean of RCI (45.7) is close to the mean reported by Houston et al. (2002) (48.5).

As the central result, I obtain:

[49] More precisely, the model predicts efforts to be equal across pairs in both periods under the baseline policy and in the first period under the partially and globally optimal policy. For second-period efforts under the partially and the globally optimal policy, the model predicts efforts to be equal only within pairs, as they depend on the realizations of first-period performance, which may differ between pairs.

[50] Because of inconsistent choices in the lottery tasks, I exclude one participant from the following analysis.

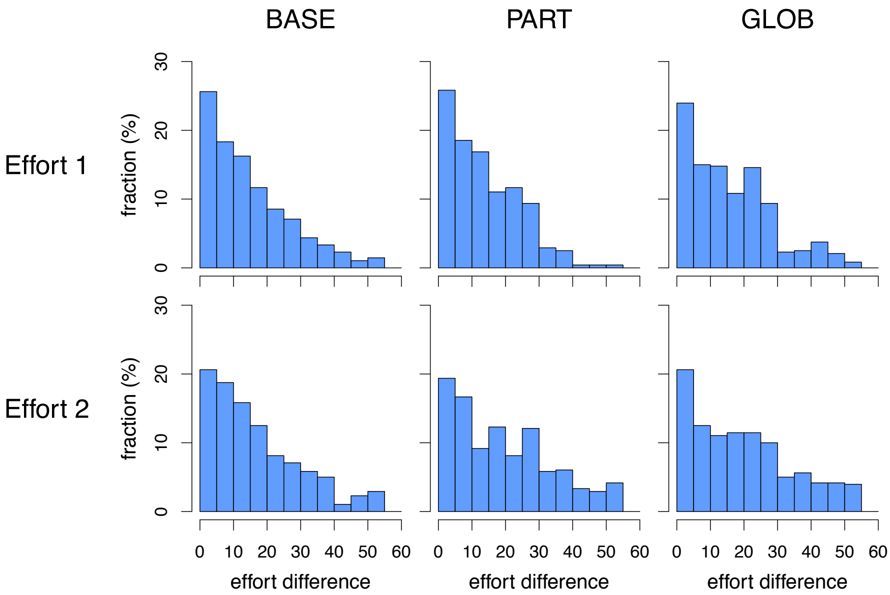


Figure 2.2: Asymmetry in effort choices. Histograms of absolute values of differences in effort choices within pairs. $N = 480$ per policy. Sample: all participants.

Result 2.6. *(i) Loss aversion affects total effort negatively under the baseline policy. (ii) Prosociality affects total effort negatively under the globally optimal policy. (iii) The measures for risk aversion and preference towards winning have no explanatory power on total effort. (iv) The effect of loss aversion and prosociality on total effort arises mainly through their effect on second-period efforts.*

Evidence for Result 2.6. The result follows from Table 2.7, which shows, separately for each policy, regressions of total effort on the measures from the pre-experimental questionnaire (Model (1) to (3)). Note that three issues complicate the estimation: (a) The censoring of total effort between 0 and 110 as a consequence of the restriction of effort choices between 0 and 55, (b) the clustering of observations within matching groups, and (c) the small number of clusters, here equal to the number of matching groups (12). To obtain unbiased coefficient estimates, issue (a) requires the Tobit model.[51] Moreover, for an unbiased estimation of the variance-covariance matrix, issue (b) requires to use cluster-robust standard errors.[52] It is well-known, however, that with few clusters (issue (c)), the conventional cluster-

[51] Specifically, I use the generalization of the Tobit model to lower and upper censoring introduced by Rosett and Nelson (1975).

[52] Clustering of the observations implies correlation in the error structure, which violates the assumption of uncorrelated errors underlying the standard (Tobit) regression model. As a consequence, the conventional estimation of the variance-covariance matrix may be biased.

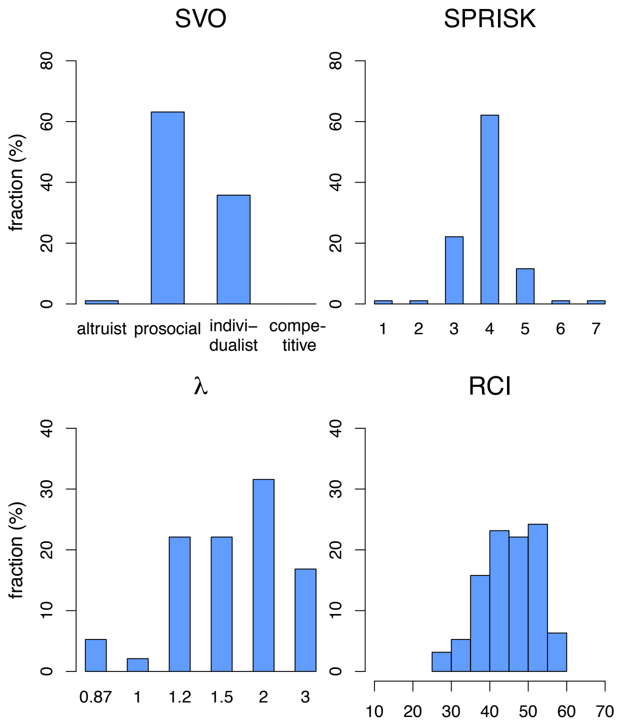


Figure 2.3: Histograms of measures from pre-experimental questionnaire. $N = 95$. Sample: All participants with non-inconsistent choices in the lottery tasks.

robust sandwich estimator[53] for the variance-covariance matrix may be biased as well (see, for example, Cameron et al., 2008). As a solution, I determine the p-values of the coefficients with the pairs cluster bootstrap-t procedure (see Appendix A2.4 for a summary). Cameron et al. (2008) show by simulation that in an ordinary least squares estimation, this procedure maintains a reasonably correct size with both clustered observations (issue (b)) and a small number of clusters (issue (c)). Fehr and Williams (2013) demonstrate how to apply the pairs-cluster bootstrap-t to a Tobit model.[54,55] Thus motivated, Table 2.7 shows that in BASE (Model (1)), the coefficient of λ is significant and negative, while the coefficients of PROSOC, SPRISK and RCI are not significantly different from 0. In PART (Model (2)), no

[53]The cluster-robust sandwich estimator is a well-known generalization of the heteroscedasticity-consistent variance-covariance estimator developed by White (1980).

[54]The wild cluster bootstrap-t performs even better in the simulations of Cameron et al. (2008). It is, however, not applicable to a Tobit model, as Fehr and Williams (2013) point out.

[55]Strictly speaking, the presence of correlated errors makes the likelihood function of the Tobit model invalid. As a consequence, the coefficient estimates may be biased. It is, however, common practice in this situation to estimate the parameters with the Tobit model and correct the standard errors afterwards. The underlying assumption is that the likelihood function of the Tobit model is still sufficiently well-specified to provide unbiased estimates of the coefficients. This corresponds to maximizing a pseudo-log-likelihood function. For a critical discussion of this approach, see, for example, King and Roberts (2014).

Table 2.7: Explanation of variation in effort choices

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dep. var.	Total effort			Effort 1			Effort 2		
Policy	BASE	PART	GLOB	BASE	PART	GLOB	BASE	PART	GLOB
CONSTANT	80.64***	56.51**	50.03**	48.14***	41.04**	24.92*	34.69***	17.80	26.14*
	(0.001)	(0.050)	(0.025)	(0.002)	(0.027)	(0.069)	(0.003)	(0.219)	(0.062)
PROSOC	-5.51	-3.88	-7.64**	-2.94*	-2.06	-3.22	-3.03	-1.96	-5.98*
	(0.132)	(0.279)	(0.050)	(0.073)	(0.464)	(0.163)	(0.211)	(0.384)	(0.068)
SPRISK	1.57	4.39	4.19	-0.05	2.79	2.38	1.46	1.50	1.76
	(0.567)	(0.200)	(0.289)	(0.974)	(0.155)	(0.277)	(0.440)	(0.369)	(0.475)
λ	-7.91**	-3.31	-4.33	-3.29	-2.78	-3.20	-4.96***	-1.05	-1.53
	(0.042)	(0.344)	(0.393)	(0.145)	(0.266)	(0.237)	(0.008)	(0.608)	(0.666)
RCI	0.02	-0.02	0.39*	-0.11	-0.20	0.24	0.13	0.14	0.19
	(0.916)	(0.938)	(0.060)	(0.319)	(0.312)	(0.117)	(0.296)	(0.492)	(0.195)
ROUND	-1.20**	-0.10	-0.21	-0.45**	0.20	0.05	-0.87***	-0.31	-0.21
	(0.015)	(0.700)	(0.458)	(0.037)	(0.293)	(0.730)	(0.000)	(0.158)	(0.398)
N	950	950	950	950	950	950	950	950	950
Left-censored	42	23	39	58	30	51	94	177	126
Right-censored	60	47	97	79	133	164	86	85	186
Clusters	12	12	12	12	12	12	12	12	12
Log-likelihood	-4215.42	-4271.19	-4176.57	-3608.09	-3485.75	-3434.95	-3574.80	-3456.78	-3325.95
Bootstrap samples	10000	10000	10000	10000	10000	10000	10000	10000	10000

Tobit regressions. Bootstrapped p-values given in parentheses, computed using pairs cluster bootstrap-t with standard errors clustered on matching group. Sample: All participants with non-inconsistent choices in lottery tasks. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

coefficient is significantly different from 0. In GLOB, the coefficient of PROSOC is significant and negative, while all other coefficients are not significantly different from 0.[56,57] This implies parts (i) to (iii). Part (iv) follows from Models (4) to (9) in Table 2.7, which are regressions of first- and second-period efforts under all three policies on the measures from the pre-experimental questionnaire. We see that under those policies in which λ and PROSOC have a significant effect on total effort, their coefficients are at least marginally significantly different from 0 for second-period efforts ((Models (7) and (9)), but not for first-period efforts (Models (4) and (6)).[58]

Result 2.6 implies that the measures for loss aversion and social value orientation obtained in the pre-experimental questionnaire contribute to explaining heterogeneity in behavior across individuals. This is consistent with previous findings on the effort-reducing effect of loss aversion in lottery contests and all-pay auctions (Kong, 2008; Jönsson, 2013; Shupp et al., 2013) and of other-regarding preferences in lottery contests (Hernandez et al., 2013).

Aside from a low statistical power of the design, the result that the measures for risk aversion and preferences towards winning do not explain variation in efforts may be due to an incorrect measurement of both phenomena.[59] This seems particularly likely in the case of risk aversion. On the one hand, as I mention in Section 2.4.2, the detrimental effect of risk aversion on efforts in tournaments is well-documented. On the other hand, the lower effect of the weight-prize adjustment than predicted seems to be consistent with risk aversion: As I argue in Appendix A2.5 with a simplified model, risk-averse subjects may actually have lower effort incentives under the globally optimal policy than under the baseline policy. This would explain why the effect of the weight-prize adjustment is lower than predicted by the theoretical model, which relies on risk neutrality.

Result 2.6 further suggests that the policy parameters affect the degree to which loss aversion and social value orientation cause variation in the behavior of the participants. Hence, one might as well expect that the effects of the policy adjustments

[56] Murphy et al. (2011) suggest the index value itself as a measure for social value orientation, arguing that it has a higher resolution than the resulting categorization. When substituting the variable PROSOC by the index value, I never obtain significance in any of the regressions.

[57] While the coefficient of RCI is marginally significant under the globally optimal policy, its effect is apparently not pronounced enough to generate explanatory power on first- and second-period efforts.

[58] I also allow for a time trend in the regressions by including the variable ROUND, which counts for how many rounds the participants have already been interacting under the current policy. The time trend is significant only under BASE, which is consistent with the observation in Figure A2.1.

[59] Of course, this may also hold for the papers finding an effect of risk aversion and/or preferences towards winning on efforts, implying that they may have found the effect of a different phenomenon than they claim. For example, the usual measure for preferences towards winning are the bids of participants in a zero-prize tournament. It is not clear whether this is a good measure for preferences towards winning, as the discussion of Brookins and Ryvkin (2014, p. 258) implies.

on total effort vary with the participants' level of loss aversion and their social value orientation. However, when testing this latter hypothesis, I obtain:

Result 2.7. *The measures from the pre-experimental questionnaire do not explain individual variation in the effects of the policy adjustments on total effort.*

Evidence for Result 2.7. The result follows from the regressions in Table 2.8. In these regressions, I relate a participant's mean change of total effort through a policy adjustment, i.e. the difference in the participant's mean total efforts under the two involved policies, to the measures from the pre-experimental questionnaire. Since the dependent variable is not censored, I use ordinary least squares regressions. We see that the measures have no explanatory power for changes in total efforts through the weight adjustment (Model (1)), nor through the weight-prize adjustment (Model (4)) or the prize adjustment (Model (7)).[60]

Result 2.7 implies that the effects of the measures from the pre-experimental questionnaire on total effort, which I identified in Result 2.6, do not differ significantly between policies.[61]

Instead of explaining absolute effort levels, the next result relates *over-expenditure* in total effort to prosociality:

Result 2.8. *(i) Under the baseline and the partially optimal policy, both prosocial and non-prosocial participants exhibit a similar over-expenditure of total effort. (ii) Under the globally optimal policy, prosocial and non-prosocial participants differ in their degree of over-expenditure. While non-prosocial participants still exhibit over-expenditure, the total efforts of prosocial participants do not differ from the prediction.*

Evidence for Result 2.8. The result follows from Table 2.9, which contains regressions of the difference between total effort and the prediction on the dummy variable PROSOC under the three policies. In these regressions, the constant measures over-expenditure of non-prosocial participants, while the coefficient of PROSOC measures by how much over-expenditure of prosocial participants differs from that of non-prosocial participants. We see that both in BASE (Model (1)) and in PART (Model (2)), the constant is positive and significant, while the coefficient of PROSOC is not significant. This implies part (i). Part (ii) follows from Model (3) and Model (4).

[60] I obtain the same result when using a Tobit model to regress efforts on policy dummies interacted with the measures from the pre-experimental questionnaire.

[61] Note that I also used the mean effects of the policy adjustments on first- and second-period efforts as the dependent variable (Models (2), (3), (5), (6), (8), (9)). The estimates are predominantly insignificant, with the exception of a few marginally significant estimates (coefficients of λ in Model (3) and of SPRISK and RCI in Model (5)). As the values of the (adjusted) R^2 are all very low in these regressions, I refrain from further interpretation.

Table 2.8: Explanation of variation in effects of policy adjustments

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dep. var.	Change in mean effort by a participant through...								
	weight adjustment			weight-prize adjustment			prize adjustment		
	Total	Effort 1	Effort 2	Total	Effort 1	Effort 2	Total	Effort 1	Effort 2
CONSTANT	-14.63	-4.96	-9.67	-23.69**	-19.24**	-4.45	-9.06	-14.28	5.22
	(0.553)	(0.647)	(0.496)	(0.047)	(0.046)	(0.549)	(0.728)	(0.393)	(0.625)
PROSOC	0.95	0.49	0.46	-1.10	0.58	-1.68	-2.06	0.09	-2.14
	(0.631)	(0.745)	(0.718)	(0.777)	(0.764)	(0.512)	(0.530)	(0.972)	(0.202)
SPRISK	2.51	2.45	0.05	2.78	2.41*	0.37	0.27	-0.04	0.32
	(0.508)	(0.103)	(0.983)	(0.228)	(0.070)	(0.767)	(0.947)	(0.985)	(0.880)
λ	4.13	0.56	3.58*	3.66	0.44	3.22	-0.48	-0.11	-0.36
	(0.208)	(0.767)	(0.057)	(0.133)	(0.786)	(0.107)	(0.909)	(0.962)	(0.877)
RCI	-0.07	-0.04	-0.03	0.28	0.28*	0.01	0.35	0.31	0.04
	(0.813)	(0.781)	(0.869)	(0.246)	(0.086)	(0.944)	(0.332)	(0.230)	(0.770)
N	95	95	95	95	95	95	95	95	95
Clusters	12	12	12	12	12	12	12	12	12
R^2	0.05	0.05	0.05	0.04	0.05	0.04	0.02	0.03	0.01
Adj. R^2	0.01	0.01	0.01	0.00	0.01	0.00	-0.02	-0.01	-0.03
Bootstrap samples	10000	10000	10000	10000	10000	10000	10000	10000	10000

Ordinary least squares regressions. Bootstrapped p-values given in parentheses, computed using pairs cluster bootstrap-t with standard errors clustered on matching group. Sample: All participants with non-inconsistent choices in lottery tasks. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

In Model (3), both the constant and the coefficient of PROSOC is significant, implying that in GLOB, non-prosocial participants exhibit over-expenditure, while over-expenditure of non-prosocial participants is significantly different. Model (4) is a regression of over-expenditure on PROSOC and the dummy variable NOPROSOC for non-prosocial participants. In this regression, the coefficient of PROSOC measures the absolute level of over-expenditure of prosocial participants in GLOB.[62] Since it is not significant, total efforts of prosocial participants do not differ from the prediction.

Table 2.9: Explanation of variation in over-expenditure

Model	(1)	(2)	(3)	(4)
Dep. var.	Over-expenditure in total effort			
Policy	BASE	PART	GLOB	GLOB
CONSTANT	21.77***	11.95**	12.14***	
	(0.002)	(0.041)	(0.006)	
PROSOC	-5.80	-4.91	-9.36**	2.77
	(0.137)	(0.224)	(0.030)	(0.298)
NOPROSOC				12.14***
				(0.006)
N	950	950	950	950
Left-censored	42	23	39	39
Right-censored	60	47	97	97
Clusters	12	12	12	12
Log-likelihood	-4233.95	-4278.10	-4185.54	-4185.54
Bootstrap samples	10000	10000	10000	10000

Tobit regressions. Dependent variable is calculated as observed total effort minus predicted total effort. Bootstrapped p-values given in parentheses, computed using pairs cluster bootstrap-t with standard errors clustered on matching group. Sample: All participants with non-inconsistent choices in lottery tasks. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Result 2.8 allows me to draw conclusions regarding the role of prosociality for the effectiveness of the globally optimal policy in increasing total efforts. The result implies that I generally observe over-expenditure among the participants, except for the case of prosocial participants under the globally optimal policy. This reduces the effect of the weight-prize adjustment on total efforts compared to a situation in which prosocial participants would behave similar to non-prosocial participants.

[62] I do not use likelihood ratio tests when testing linear combinations of the coefficients because they would not take the clustering in the observations into account.

Hence, although I cannot show that the effect of the weight-prize adjustment is lower among prosocials (Result 2.7), it still seems very likely that one reason for the overall small effect of the globally optimal policy is that it induces particularly low efforts among prosocial participants.

What motivates prosocial participants to exert systematically lower effort under the globally optimal policy than non-prosocial participants? Note that the globally optimal policy induces a higher degree of ex-post income inequality than the baseline and the partially optimal policy. Indeed, under the globally optimal policy, there is an income difference of CHF 30 between both participants, as only one of both can win the overall prize of CHF 30. Under the baseline and the partially optimal policy, however, it is also possible that the first- and second-period prize go to different participants, so that an equal distribution of prizes becomes possible. Therefore, it may be the case that the high degree of ex-post income inequality under the globally optimally policy reduces incentives for those subjects who have an aversion against income inequality that benefits themselves (aheadness averse subjects). Since aheadness averse subjects are likely to to turn out as procoscials in the SVO Slider Measure, this would provide an explanation for lower efforts of prosocial participants under the globally optimal policy. Aheadness aversion indeed seems to play a role in tournament-like incentive systems: As Bartling et al. (2009) find, aheadness averse subjects self-select systematically less into competition than non-aheadness averse subjects.

However, it is not at all clear how aheadness aversion would interact with another form of inequality aversion, that is aversion against *disadvantageous* inequality (behindness aversion). Subjects who are both aheadness and behindess averse, but dislike disadvantageous inequality more than they dislike advantageous inequality, may actually exert *more* effort under a policy with high ex-post income inequality than subjects who are not inequality averse. This discussion highlights the need for a formal model of inequality aversion when explaining the observations for prosocial participants under the globally optimal policy, which I leave to future research. Furthermore, the discussion also suggests that in the elicitation of social preferences, it would be helpful to use the full version of the SVO Slider Measure. The full version uses nine additional dictator games to distinguish prosocial individuals further according to their type of inequality aversion. Such information would likely help when forming hypotheses about the effects of the globally optimal policy on prosocial participants.[63]

Finally, the above results also suggest that the policy identified as optimal by the theoretical model is not necessarily the optimal policy for a tournament among real subjects. Note that the theoretical model neglects both loss aversion and social preferences. However, the measures for both phenomena explain parts of the varia-

[63]It was not foreseeable at the time of the experiment that the full version would turn out to be the preferred elicitation method.

tion in efforts, suggesting that loss aversion and social preferences play a role in the behavior of the participants. Therefore, the optimal policy for real subjects is likely to differ from the one derived in this study. Nevertheless, the theoretical model still provides reasonable guidance for the optimal design of incentives, as it proved effective in increasing efforts despite of behavioral variation among the participants (see Result 2.1).

To sum up, this part focuses on the determinants of heterogeneity in behavior and the implications for the design of the policy. After establishing a substantial degree of heterogeneity in behavior among the participants (Result 2.5), I show that under the baseline policy (the globally optimal policy), loss-aversion (prosociality) decreases total effort through an effect on second-period efforts (Result 2.6). Furthermore, I find no explanatory power of loss aversion and prosociality on variation in the effects of the policy adjustments (Result 2.7). However, I show that prosocial and non-prosocial participants exhibit a similar degree of over-expenditure under the baseline and the partially optimal policy, while under the globally optimal policy, prosocials, in contrast to non-prosocials, do not exhibit over-expenditure (Result 2.8). This leads me to the conjecture that one reason for the smaller effect of the weight-prize adjustment compared to the prediction is that it induces particularly low efforts among prosocial participants. I further argue that the optimal policy derived from the theoretical model may not be the optimal policy in practice.

2.5.3 Understanding the dynamics

In this part, I investigate the effect of first-period outcomes on behavior in the second period. Recall that the core of the optimal policy is the positive weight of past performance in the second period. As argued above, the positive weight gives additional incentives for first-period efforts, but introduces asymmetry between the agents in the second period by giving a headstart of $\eta\Delta s_{i1}$ to the agent with $s_{i1} > s_{j1}$. As, by (2.3), second-period efforts depend negatively on the size of the relative first-period performance Δs_{i1}, this asymmetry reduces incentives in the second period. I now test whether the observed behavior is consistent with this prediction.

In contrast to the theoretical prediction, first-period outcomes also have an effect on behavior in the second period under the baseline policy, in which the weight of past performance is zero:

Result 2.9. *(i) Under the baseline policy, second-period efforts decrease in the size of a positive relative performance ($\Delta s_{i1} > 0$) and increase in the size of a negative relative performance ($\Delta s_{i1} < 0$). (ii) This effect is entirely driven by participants identified as prosocial in the pre-experimental questionnaire.*

Evidence for Result 2.9. Table 2.10 supports the result. It contains regressions of second-period efforts on the size of the relative first-period performance Δs_{i1} and various interaction terms. To avoid endogeneity of Δs_{i1} through its dependence on first-period effort e_{i1}, I include e_{i1} as a control variable.[64] Model (1) is a regression of second-period efforts in BASE on $|\Delta s_{i1}|$ and on $|\Delta s_{i1}|_{\Delta s_{i1}<0}$, which is an interaction of $|\Delta s_{i1}|$ with a dummy variable indicating $\Delta s_{i1} < 0$. In this regression, the coefficient of $|\Delta s_{i1}|$ measures the effect of a positive relative performance, while the sum of the coefficients of $|\Delta s_{i1}|$ and $|\Delta s_{i1}|_{\Delta s_{i1}<0}$ measures the effect of a negative relative performance. We see that the coefficient of $|\Delta s_{i1}|$ is significant and negative, while the sum of the coefficients of $|\Delta s_{i1}|$ and $|\Delta s_{i1}|_{\Delta s_{i1}<0}$ is positive (0.05). Model (2) shows that it is also significant. Instead of $|\Delta s_{i1}|$, this regression includes $|\Delta s_{i1}|_{\Delta s_{i1}>0}$, which is an interaction of $|\Delta s_{i1}|$ with a dummy variable indicating $|\Delta s_{i1}| > 0$. In this regression, the coefficient of $|\Delta s_{i1}|_{\Delta s_{i1}<0}$ measures the effect of a negative relative performance and is significant. This supports part (i). Part (ii) follows from Model (3). It extends Model (1) by $|\Delta s_{i1}| \cdot$PROSOC and $|\Delta s_{i1}|_{\Delta s_{i1}<0} \cdot$PROSOC, which are interactions of $|\Delta s_{i1}|$ and of $|\Delta s_{i1}|_{\Delta s_{i1}<0}$ with PROSOC, the dummy variable indicating that the participant was characterized as prosocial in the pre-experimental questionnaire. In this regression, the coefficients of $|\Delta s_{i1}|$ and $|\Delta s_{i1}|_{\Delta s_{i1}<0}$ measure the effect of a positive and negative relative performance on non-prosocial participants, while the coefficients of $|\Delta s_{i1}| \cdot$PROSOC and $|\Delta s_{i1}|_{\Delta s_{i1}<0} \cdot$PROSOC measure by how much the effects differ for prosocial participants. We see that the coefficients of $|\Delta s_{i1}|$ and of $|\Delta s_{i1}|_{\Delta s_{i1}<0}$ are never significant, while the coefficients of $|\Delta s_{i1}| \cdot$PROSOC and $|\Delta s_{i1}|_{\Delta s_{i1}<0} \cdot$PROSOC are significant. Hence, the relative first-period performance has no effect on non-prosocial participants, while its effect on prosocial participants significantly differs from this zero effect. This implies that the effect of relative first-period performance on second-period efforts in BASE results only from prosocial participants.

Apparently, the game has a dynamic nature for the participants also under the baseline policy, although both periods are independent of each other from a monetary utility perspective. Since only prosocial participants drive the result, it is likely that inequality aversion plays a role.[65] In fact, the directions of the dynamic effects under the baseline policy are in line with this conjecture. To see this, note that a participant with a positive (negative) relative first-period performance is the winner (loser) of the first-period prize W_1. My results imply that for prosocial participants, second-period efforts of first-period winners are lower than second-period efforts of

[64] Δs_{i1} is endogenous by definition, as $\Delta s_{i1} = e_{i1} + \varepsilon_{i1} - s_{j1}$. With e_{i1} as a control variable, Δs_{i1} captures only the variation that is caused by the participant's first-period observation error ε_{i1} and the opponent's first-period performance Δs_{j1}. As both are exogenous to the decision problem of the participant, so is Δs_{i1} when controlling for e_{i1}. Hence, in Table 2.10, the coefficient of Δs_{i1} is an unbiased estimate of the causal effect of Δs_{i1} on second-period efforts.

[65] Recall that subjects identified as prosocial in the pre-experimental questionnaire can be both inequality tolerant and inequality averse.

Table 2.10: Effect of first-period asymmetry on second-period efforts

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. var.	Effort 2						
Policy	BASE	BASE	BASE	PART	PART	GLOB	GLOB
CONSTANT	2.88	2.88	3.27	12.33***	12.51***	14.64***	14.74***
	(0.316)	(0.316)	(0.244)	(0.005)	(0.004)	(0.002)	(0.002)
EFFORT 1	0.86***	0.86***	0.85***	0.58***	0.57***	0.85***	0.85***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
$\lvert\Delta s_{i1}\rvert$	-0.07***		0.01	-0.20***	-0.16**	-0.30***	-0.24***
	(0.006)		(0.819)	(0.000)	(0.011)	(0.000)	(0.001)
$\lvert\Delta s_{i1}\rvert \cdot$ PROSOC			-0.12**		-0.05		-0.09
			(0.039)		(0.302)		(0.124)
$\lvert\Delta s_{i1}\rvert_{\Delta s_{i1}>0}$		-0.07***					
		(0.006)					
$\lvert\Delta s_{i1}\rvert_{\Delta s_{i1}<0}$	0.12***	0.05**	0.00	-0.07	-0.09	-0.09*	-0.16*
	(0.000)	(0.030)	(0.946)	(0.133)	(0.271)	(0.072)	(0.080)
$\lvert\Delta s_{i1}\rvert_{\Delta s_{i1}<0} \cdot$ PROSOC			0.18***		0.05		0.10
			(0.009)		(0.686)		(0.205)
N	950	950	950	950	950	950	950
Left-censored	94	94	94	177	177	126	126
Right-censored	86	86	86	85	85	186	186
Clusters	12	12	12	12	12	12	12
Log-likelihood	-3375.33	-3375.33	-3367.07	-3372.44	-3371.78	-3108.25	-3106.20
Bootstrap samples	10000	10000	10000	10000	10000	10000	10000

Tobit regressions. Bootstrapped p-values given in parentheses, computed using pairs cluster bootstrap-t with standard errors clustered on matching group. Sample: All participants with non-inconsistent choices in lottery tasks. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

first-period losers. Such behavior reduces the expected inequality after the second period and is, therefore, consistent with inequality aversion.[66,67]

For the partially and the globally optimal policy, I obtain:

Result 2.10. *(i) Both under the partially and under the globally optimal policy, second-period efforts decrease in the size of the relative first-period performance Δs_{i1}. (ii) Under the partially optimal policy, a positive relative first-period performance ($\Delta s_{i1} > 0$) reduces efforts just as high as an equally sized negative relative first-period performance ($\Delta s_{i1} < 0$). Under the globally optimal policy, a positive relative first-period performance ($\Delta s_{i1} > 0$) decreases efforts less than an equally sized negative relative first-period performance ($\Delta s_{i1} < 0$). (iii) Both under the partially and the globally optimal policy, the behavior of prosocial and non-prosocial participants does not differ.*

Evidence for Result 2.10. The result follows from Models (4) to (7) in Table 2.10. Models (4) and (6) are regressions of second-period efforts in PART and GLOB on e_{i1}, $|\Delta s_{i1}|$ and $|\Delta s_{i1}|_{\Delta s_{i1}<0}$. In both models, the coefficient of $|\Delta s_{i1}|$ is negative and significant, which implies part (i). The coefficient of $|\Delta s_{i1}|_{\Delta s_{i1}<0}$ is not significantly different from 0 in PART, but negative and marginally significant in GLOB. Hence, in PART, the effect of a negative relative performance on second-period efforts is not significantly different from the effect of a positive relative performance. In GLOB, however, a negative relative performance decreases efforts significantly more than a positive relative performance. This supports part (ii). Models (5) and (7) extend Models (4) and (6) by $|\Delta s_{i1}| \cdot$PROSOC and $|\Delta s_{i1}|_{\Delta s_{i1}<0} \cdot$PROSOC as additional explanatory variables. In both models, their coefficients are not significantly different from 0, showing that the effects do not differ for prosocial participants. This implies part (iii).

According to Result 2.10, the experimental data confirms the theoretical prediction for the effect of relative first-period performance on second-period efforts under the partially and the globally optimal policy: A higher size of the relative performance decreases efforts in the second period, no matter of whether participants were ahead or behind in the first period. However, under the globally optimal policy, I identify an additional demotivation effect of being behind: A negative relative performance demotivates the participants more than an equally sized positive relative performance. In contrast to my result for the baseline policy, I do not identify a

[66] Inequality aversion as an explanation for the observed second-period behavior would be consistent with results from Drago and Heywood (1991), who find no effect of previous losing on efforts in a tournament against an automaton.

[67] I cannot rule out that reciprocity also plays a role in the observed behavior. However, note that I obtain the measure for prosociality from the behavior in a dictator game in which reciprocity is irrelevant. Hence, the explanatory power of prosociality is independent of the role of reciprocity.

systematically different behavior of prosocial individuals. In particular, unlike under the baseline policy, prosocial participants do not exhibit an inequality-reducing behavior in the second period by exerting less effort when they were ahead in the first period and more effort when they were behind in the first period. Hence, in case that inequality aversion drives the observations for the baseline policy, the monetary link between the first and the second period through a positive weight of past performance seems to mitigate such motives in the reaction to first-period outcomes.

Note that the result on the effect of relative first-period performance under the globally optimal policy is consistent with the findings of Ederer and Fehr (2007), who study the issue of performance revelation in an experiment that is very similar to the globally optimal policy. They also find that the size of the relative first-period performance decreases efforts in the second period, while their estimates suggests that a positive relative performance decreases efforts less than negative relative performance.[68,69] Moreover, my findings are consistent with results from experiments on asymmetric tournaments.[70]

In summary, this part analyzes the effect of first-period asymmetry on second-period efforts. Result 2.9 shows that under the baseline policy, the game has a dynamic nature for prosocial participants, although both periods are independent from a monetary utility perspective. More specifically, prosocials exert more effort when they lost in the first period and less effort when they won, which is consistent with inequality aversion. Result 2.10 demonstrates that under the partially and the globally optimal policy, the behavior of both prosocial and non-prosocial participants is in line with the theoretical model, as a higher size of first-period asymmetry reduces second-period efforts. I thus conjecture that the monetary link between the first and second period through the weight adjustment mitigates the effect of inequality aversion on behavior under the baseline policy. Result 2.10 further identifies an additional demotivation effect of being behind under the globally optimal policy, as a negative performance difference reduces efforts more than an equally sized positive performance difference.

[68] Ederer and Fehr (2007) do, however, not test whether both effects are different.

[69] The experiment of Ludwig and Lünser (2012) shares features of the globally optimal policy, but differs from the framework in this study in that there is no observation noise in the first period. They find that second-period efforts decrease in the size of the first-period *effort* difference, while the effect is stronger for a negative effort difference than for a positive effort difference.

[70] The negative effect of relative first-period performance on second-period efforts is consistent with experimental papers showing that efforts in tournaments among asymmetric participants are lower than efforts in tournaments among symmetric participants (see, for example, Schotter & Weigelt, 1992; Fonseca, 2009). Note that similar observations can be made in the field: Brown (2011) uses data from golf tournaments and shows that the presence of highly skilled players reduces the average performance of other players in the tournament.

2.6 Conclusion

In this study, I analyze experimentally the effects of the weight of past performance and of the spread of prize money in dynamic tournaments. The experiment compares efforts under a baseline policy of two independent tournaments to efforts when the spread of prize money and/or the weight correspond to the optimal solutions characterized by Klein and Schmutzler (2014). Only the policy adjustment that implements both the optimal weight and the optimal prize spread increases efforts. However, its effect is smaller than predicted. By relating the measures from a pre-experimental questionnaire to behavior in the laboratory, I argue that one explanation for the small effect of the optimal policy compared to the prediction is that it induces particularly low efforts among prosocial participants.

The study provides insights that are relevant for the design of competitive incentive systems. It shows that the weight of past performance and the spread of prize money significantly affect behavior in dynamic tournaments, and, therefore, are important dimensions to consider in practice. Furthermore, the study has two more general implications: First, qualitative predictions from theoretical models on relative incentives are empirically relevant. Second, for quantitative predictions, it is important to not only rely on simple material utility models, but to have a closer look at individuals' preferences.

However, it should be noted that the experiment relies on a specific parameterization of observation noise and effort costs. This parameterization yields an intriguingly simple solution: In the optimal policy, the best-performing agent across both periods receives all the money. This simplicity makes the policy seem attractive for tournaments in general. In fact, many theoretical and experimental papers that consider the issue of performance revelation rely on such a structure without further justification. Nevertheless, the general analysis of Klein and Schmutzler (2014) shows that the predictions for the optimal size of the weight and for the spread of prize money crucially depend on the noise and cost parameters. In real working environments, observation noise and effort costs are likely to be different, if not unknown. Furthermore, the behavioral phenomena identified in this study, and many more issues like social preferences towards the principal, attrition, or simply asymmetric abilities, are supposedly highly relevant for the behavior of employees. An experiment that tests the effects of this policy in a field environment would therefore be an important further step.

Acknowledgements

I am grateful to Andreas Beerli, Jean-Michel Benkert, Dan Burghart, Alain Cohn, Donja Darai, Somnath Datta, Florian Ederer, Ronald Indergand, Hedda Herdis Klein, Patrick Kline, Igor Letina, John Morgan, Yagiz Özdemir, Andrea Portmann, Armin Schmutzler, Frédéric Schneider, Carmit Segal, Roberto Weber, Tony Williams, and seminar participants in Schaffhausen (Zurich Workshop on Economics) and Zurich (Project Design Seminar, Zurich-Basel Workshop in Micro Theory) for helpful discussions and suggestions. Furthermore, I gratefully acknowledge financial support of the Swiss National Science Foundation (grant numbers 131854 and 151688).

Appendix

A2.1 Figures

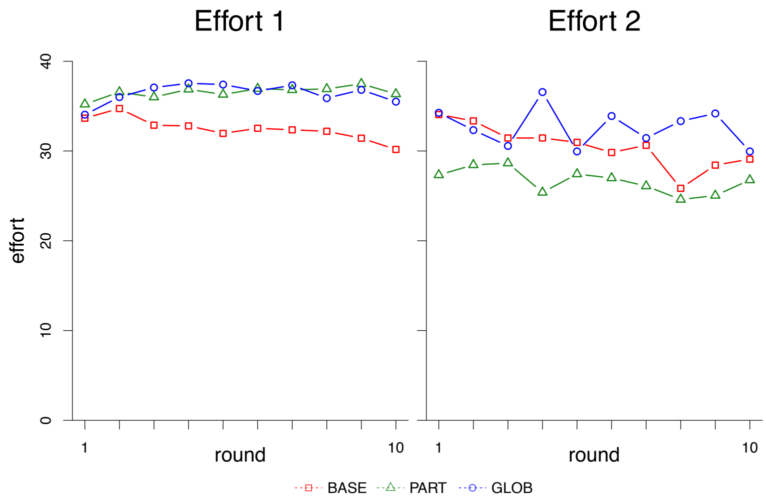


Figure A2.1: Means of efforts over rounds. $N = 96$ per round and policy. Sample: All participants.

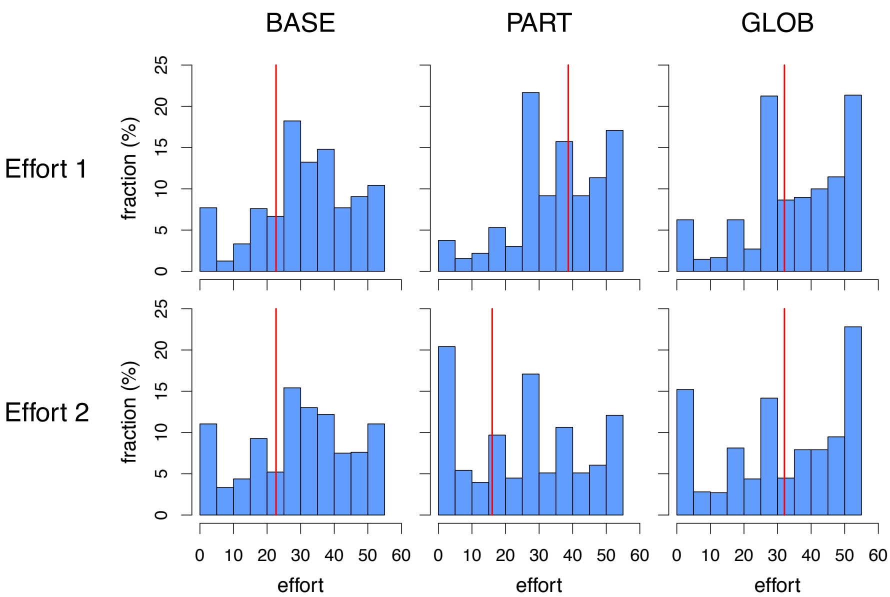


Figure A2.2: Histograms of effort choices. Red lines are Nash predictions for (expected) efforts. $N = 960$ per policy. Sample: All participants.

A2.2 Derivations

This section relies on several results provided in Chapter 1.

A2.2.1 Second-order conditions in the second period

Section A1.1.1 (proof of part (ii)) derives (A1.1) as the condition under which (2.2) characterizes the unique equilibrium efforts in the second period. Using $f_2(s) = \phi(s)$ and $K_2(e) = \frac{k}{2}(e)^2$ in (A1.1), I can write this condition as

$$\phi'(\eta \Delta s_{i1} + e_{i2} - e_{j2}) W_2 < k \;\forall\; \Delta s_{i1} \in \mathbb{R};\; \forall\; e_{i2}, e_{j2} \in \mathbb{R}^+. \tag{A2.1}$$

Section A1.6.1 (ii) derives (A1.37) as a sufficient condition for (A2.1) to hold. With $\sigma_1 = \sigma_2$, (A1.37) becomes

$$k > \frac{W}{\sigma^2 \sqrt{2\pi \exp(1)}}.$$

A2.2.2 Expected second-period payoff

Using $\sigma_1 = \sigma_2$ in (A1.33) gives

$$C(\eta) = \frac{1}{\sigma\sqrt{2\pi}\sqrt{1+\eta^2}}. \tag{A2.2}$$

With (A2.2), (A1.3) yields (2.5).

A2.2.3 Second-order conditions in the first period

Section A1.1.3 (proof of part (ii)) derives (A1.8) as the condition under which (2.6) describes the unique symmetric equilibrium in the first period. With $f_t(s) = \phi(s)$ and $K_t(e) = \frac{k}{2}(e)^2$, (A1.8) becomes

$$\begin{aligned} &\phi'(e_{i1} - e_{j1}) W_1 + \eta W_2 \int_0^\infty \phi(\eta s) \left[\phi'(s + e_{i1} - e_{j1}) - \phi'(s - e_{i1} + e_{j1})\right] ds \\ &\qquad + \frac{\eta W_2^2}{k} \int_0^\infty \phi(\eta s) \phi'(\eta s) \left[\phi'(s + e_{i1} - e_{j1}) + \phi'(s - e_{i1} + e_{j1})\right] ds < k. \end{aligned} \tag{A2.3}$$

Section A1.6.1 (ii) shows that (A1.39) is a sufficient condition for (A2.3) to hold. With $\sigma_1 = \sigma_2$, I can write (A1.39) as

$$k > \frac{W}{\sigma^2 \sqrt{2\pi \exp(1)}} + \frac{W^2}{k\sigma^4 (2\pi)^{\frac{3}{2}} \sqrt{\exp(1)}}.$$

A2.2.4 Expected second-period efforts

Using $\sigma_1 = \sigma_2$ in (1.21) yields (2.7).

A2.3 Details on pre-experimental questionnaire

A2.3.1 Details on measure for social value orientation

The SVO Slider Measure developed by Murphy et al. (2011) consists of a sequence of six dictator games. In each dictator game, the participants have to choose one of nine allocations in terms of payoffs for themselves and another participant (see Table A2.1 and Figure A2.3). As an example, game 5 involves the distribution of a total surplus of 150 points between oneself and the other.[71] The allocations are constructed in a way that in each dictator game, each of the classical types of social value orientation either strictly prefers exactly one of the allocations, or is wholly indifferent between all of them.

Table A2.1: Possible allocation choices in SVO Slider Measure

Game #	Receiver	Allocation #								
		1	2	3	4	5	6	7	8	9
1	oneself	85	85	85	85	85	85	85	85	85
	other	85	76	68	59	50	41	33	24	15
2	oneself	85	87	89	91	93	94	96	98	100
	other	15	19	24	28	33	37	41	46	50
3	oneself	50	54	59	63	68	72	76	81	85
	other	100	98	96	94	93	91	89	87	85
4	oneself	50	54	59	63	68	72	76	81	85
	other	100	89	79	68	58	47	36	26	15
5	oneself	100	94	88	81	75	69	63	56	50
	other	50	56	63	69	75	81	88	94	100
6	oneself	100	98	96	94	93	91	89	87	85
	other	50	54	59	63	68	72	76	81	85

Source: Adapted from Murphy et al. (2011).

The categorization of the participants into one of the orientation types is based on their choices in these dictator games. Let $\overline{A}_o$ be the average of what the participant allocated to the other across the six games and $\overline{A}_s$ the average of what the participant

[71]Note that in this dictator game, the price of giving is 1. In the other games, the price of giving is not equal to 1, so that the total surplus varies between choices.

allocated to him/herself. Murphy et al. (2011) then define a participant's SVO index as

$$\text{SVO} = \arctan\left(\frac{\overline{A}_o - 50}{\overline{A}_s - 50}\right).$$

Intuitively, in Figure A2.3, the index corresponds to the angle at vertex $(50, 50)$ between the average allocation chosen and the horizontal. Note that each classical type would generate a particular index value when faced with the dictator games. Following Murphy et al. (2011), I determine the participants' social value orientation type as the classical orientation type whose index value their own index value is closest to. Table A2.2 contains the index values implied by orientation types and the resulting intervals for the empirical categorization.[72]

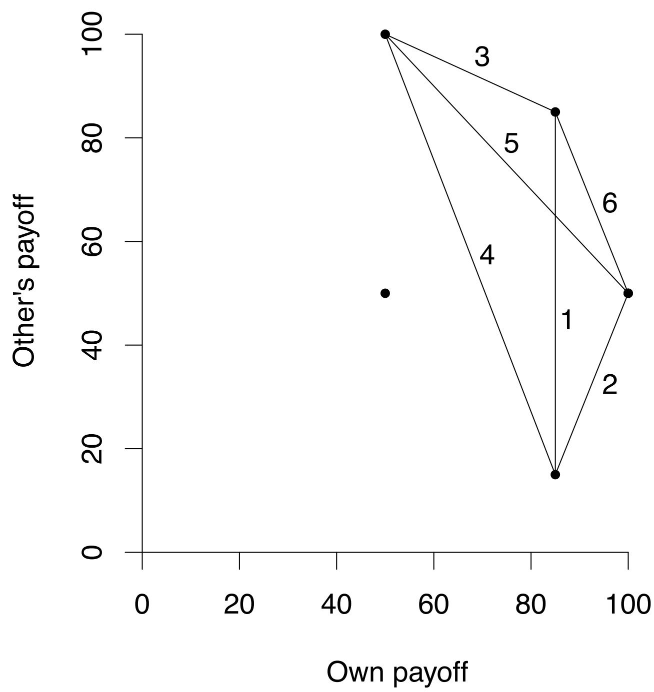


Figure A2.3: Allocations in the SVO Slider Measure. The lines connect the allocations in the six dictator games given in Table A2.1. Source: Adapted from Fehr and Williams (2013).

I implemented the SVO Slider Measure in the following way. Before making decisions in the dictator games, the participants were instructed that after the completion of the questionnaire, they would be randomly paired with another participant, and that one of the 12 decisions made by both participants in this pair would

[72]Note that there is a range of possible values for prosocials and individualists. This is due to the fact that both orientation types are indifferent between the allocations in one of the six dictator games each.

Table A2.2: Characterization of SVO types

Orientation type	Implied index value	Range for characterization
Competitive	$-16.26°$	$\leq -12.04°$
Individualist	$\in [-7.82°, 7.82°]$	$(-12.04°, 22.45°]$
Prosocial	$\in [37.09°, 52.91°]$	$(22.45°, 57.15°]$
Altruist	$61.39°$	$> 57.15°$

Source: Adapted from Murphy et al. (2011).

then be randomly chosen to determine their payoffs. Then, every game appeared separately on the participants' computer screen. For every participant, the order of presentation was randomly determined. All payoffs were expressed in CHF, using an exchange rate of CHF 1 per 10 points as given in Table A2.1.

A2.3.2 Details on measure for risk aversion

In the lottery task, the safe payoff varied between CHF $X \in \{2, 3, 4, 5, 6, 7\}$. The lottery yielded CHF 10 or CHF 0 with equal chances. The choice situations were presented on one screen, ordered in decreasing value of the safe payoff (see Table A2.3).

Table A2.3: Lottery task to elicit risk aversion

Situation #	Safe payoff	Lottery
1	CHF 7	50%: CHF 10, 50%: CHF 0
2	CHF 6	50%: CHF 10, 50%: CHF 0
3	CHF 5	50%: CHF 10, 50%: CHF 0
4	CHF 4	50%: CHF 10, 50%: CHF 0
5	CHF 3	50%: CHF 10, 50%: CHF 0
6	CHF 2	50%: CHF 10, 50%: CHF 0

I infer the participants' degree of risk aversion from the position at which they switched from choosing the safe amount to choosing the lottery. To this end, I use a similar argument as Dohmen et al. (2011): Since the expected value of the lottery is CHF 5, risk-loving subjects should switch before situation 3 (at which the safe payoff equals the expected value of the lottery), and risk-averse subjects after situation 3. Hence, the later an individual switches, the higher is the underlying degree of risk aversion. I thus define the variable SPRISK as the number of the situation at which the participants chose the lottery for the first time.[73,74]

After the completion of the questionnaire, the computer randomly selected one of the six choice situations for each participant (Cubitt et al., 1998). The participants' payoff then followed from their decision for the selected situation – if they had chosen the safe payoff, the payoff was equal to the safe payoff, while if they had chosen the lottery, the payoff was randomly chosen between CHF 10 and CHF 0.

A2.3.3 Details on measure for loss aversion

In this task, which was developed by Gächter et al. (2010), the participants have to make decisions for six choice situations involving a safe payoff of CHF 0 and a lottery. The lotteries yields, with equal chances, a payoff of CHF 6 or a payoff of CHF $-X$, while $X \in \{2, 3, 4, 5, 6, 7\}$ (see Table A2.4).

Table A2.4: Lottery task to elicit loss aversion

Situation #	Safe payoff	Lottery
1	CHF 0	50%: CHF 6, 50%: CHF -2
2	CHF 0	50%: CHF 6, 50%: CHF -3
3	CHF 0	50%: CHF 6, 50%: CHF -4
4	CHF 0	50%: CHF 6, 50%: CHF -5
5	CHF 0	50%: CHF 6, 50%: CHF -6
6	CHF 0	50%: CHF 6, 50%: CHF -7

Source: Adapted from Gächter et al. (2010).

The participants' level of loss aversion follows from the point at which they start rejecting the lottery in favor of the safe amount. Following Gächter et al. (2010), a decision maker is indifferent between accepting and rejecting a lottery that yields a gain of G and a loss of L with equal chances if

$$G = \lambda \cdot L.$$

[73] With this definition, a participant who always chose the lottery – and is thus very risk-loving – receives a value of 1, which is an upper bound for the switching point in the hypothetical case that there were additional situations above situation 1 with a safe payoff of more than CHF 7. For a participant who never chose the lottery – and is thus very risk-averse – I set SPRISK to 7, which is a lower bound for the switching point in the hypothetical case that there were additional situations below situation 6 with a safe payoff of less than CHF 2.

[74] The measure which I use is a linear transformation of the measure of Dohmen et al. (2011), who present the situations in increasing size of the safe amount and set the measure of risk aversion equal to the value of the safe payoff in the situation at which the participants switch from choosing the lottery to choosing the safe payoff.

Gächter et al. (2010) define λ as the coefficient of loss aversion. They argue that $\lambda > 1$ implies loss aversion, as losses are weighted more heavily than equally sized gains.[75] Gächter et al. (2010) then calculate a participant's λ as

$$\lambda = \frac{G^a}{L^a},$$

where G^a and L^a are the gain and the loss of the lottery with the highest loss that is still accepted by a participant. Note that if a participant always (never) rejected the lottery, I can only determine a lower (upper) bound for λ.[76] Furthermore, if a participant was inconsistent and rejected a first lottery but accepted a second that would have yielded a higher loss than the first, it is not possible to determine λ. Table A2.5 shows the values for λ that follow from the possible choices in the lottery task.

Table A2.5: Possible choices and implied values for λ

Choice	implied λ
Always reject	> 3
Accept #1, reject #2–#6	3
Accept #1 – #2, reject #3–#6	2
Accept #1 – #3, reject #4–#6	1.5
Accept #1 – #4, reject #5–#6	1.2
Accept #1 – #5, reject #6	1
Never reject	≤ 0.87

Source: Adapted from Gächter et al. (2010).

As for the lottery task to elicit risk aversion, the computer randomly selected one of the six choice situations for each participant after the completion of the questionnaire (Cubitt et al., 1998) and determined the payoffs according to the decisions for the selected situation.

A2.3.4 Details on measure for competitiveness

The Reversed Competitiveness Index developed by Houston et al. (2002) consists of 14 statements about competition in daily life contexts (see Table A2.6). The participants state on a scale from 1 (strongly disagree) to 5 (strongly agree) how they agree with each statement (Likert, 1932). According to the definition of Houston et al. (2002), a participant's RCI value equals the sum of the individual answers and

[75] This is a special case of the more general model of Tversky and Kahneman (1992), who allow for probability weighting and nonlinear utility.

[76] In these cases, I set λ to 3 or 0.87, respectively.

ranges between 14 and 70.[77] In the pre-experimental questionnaire, the questions appeared separately on the screen in a randomly determined order.

Table A2.6: Statements in Revised Competitiveness Index

#	Statement
1	I like competition.
2	I am a competitive individual.
3	I enjoy competing against an opponent.
4	I don't like competing against other people.
5	I get satisfaction from competing with others.
6	I find competitive situations unpleasant.
7	I dread competing against other people.
8	I try to avoid competing with others.
9	I often try to out perform others.
10	I try to avoid arguments.
11	I will do almost anything to avoid an argument.
12	I often remain quiet rather than risk hurting another person.
13	I don't enjoy challenging others even when I think they are wrong.
14	In general, I will go along with the group rather than create conflict.

Source: Adapted from Houston et al. (2002).

A2.4 The pairs-cluster bootstrap-t procedure

The following summary relies on Cameron et al. (2008, p. 427). Suppose there are C clusters. The pairs-cluster bootstrap-t procedure starts with an ordinary least squares estimation of coefficient j, $\hat{\beta}_j$, and a cluster-robust estimation of its standard error, $s_{\hat{\beta}_j}$, based on the whole sample. In the next step, B bootstrap replications are executed. In each replication, the procedure samples C clusters (i.e., all of the observations from that cluster) with replacement from the original sample of clusters and calculates t-statistics based on the resampled data using cluster-robust standard errors. Precisely, let $\hat{\beta}_{j,b}$ be the ordinary least squares estimate of the coefficient in the bth replication and $s_{\hat{\beta}_{j,b}}$ the cluster-robust estimate of its standard error. The bth t-statistic is then defined as

$$t_{j,b} = \frac{\hat{\beta}_{j,b} - \hat{\beta}_j}{s_{\hat{\beta}_{j,b}}}.$$

[77] The answers to statements 4, 6, 7, 8, 11, 12, 13 and 14 are reverse-coded in the calculation of the overall score.

In the last step, the p-value for parameter j results from comparing the regular t-statistic

$$t_j = \frac{\hat{\beta}_j}{s_{\hat{\beta}_j}}$$

to the empirical distributions of the $t_{j,b}$. Note that in some of my regressions, I determine the estimates of the coefficients with the Tobit model instead of ordinary least squares.[78,79]

A2.5 Effort incentives with risk aversion

To keep the analysis tractable, I make two simplifying assumptions. First, I assume that the agents choose first- and second-period efforts simultaneously before the beginning of the first period. This corresponds to a policy in which the agents receive feedback on their first-period performance only after they have chosen their second-period efforts. The literature calls this a *no-revelation* policy, in contrast to the full revelation policy that underlies this study. As Klein and Schmutzler (2014) demonstrate, expected efforts do not differ between a no revelation and a full revelation policy if effort costs are quadratic. Second, I assume that in each period, the agents can only choose zero effort or a positive effort level of $\overline{e} > 0$. Let $K(\overline{e}) := \overline{K}$. Quadratic costs imply $\overline{K} > 0$.

Suppose agent j chooses $e_{j1} = e_{j2} = \overline{e}$. Under the baseline policy, the probability that agent i wins the prize in period t, $W_t = \frac{W}{2}$, when also choosing $e_{i1} = e_{i2} = \overline{e}$ is 0.5. Similarly, under the globally optimal policy, the probability that agent i wins the second-period prize, $W_2 = W$, when choosing $e_{i1} = e_{i2} = \overline{e}$ is 0.5. I denote the ex-post monetary payoff of agent i in period t by π_{it} and the overall monetary payoff of agent i from both periods by π_i. Under the baseline policy, the expected value of the monetary payoff of agent i in period t for $e_{it} = e_{jt} = \overline{e}$ is

$$E(\pi_{it})_B = 0.5 \cdot W_t - \overline{K} = \frac{W}{4} - \overline{K},$$

while its variance is

$$V(\pi_{it})_B = 0.5 \cdot \left(W_t - \overline{K} - E(\pi_{it})_B\right)^2 + 0.5 \cdot \left(-\overline{K} - E(\pi_{it})_B\right)^2 = \frac{W^2}{16}.$$

[78] I implemented the bootstraps in R (R Core Team, 2014) using the AER (Kleiber & Zeileis, 2008), censReg (Henningsen, 2013), doBy (Højsgaard & Halekoh, 2012), doMC (Calaway & Weston, 2014a), doRNG (Gaujoux, 2014), foreach (Calaway & Weston, 2014b), lmtest (Zeileis & Hothorn, 2002), sampling (Tillé & Matei, 2013) and sandwich (Zeileis, 2004) packages.

[79] For the specification of the cluster-robust sandwich estimator in the maximum likelihood case see, for example, King and Roberts (2014).

Therefore, under the baseline policy, expected value and variance of the monetary payoff from both periods for $e_{it} = e_{jt} = \overline{e}$ are[80]

$$E(\pi_i)_B = E(\pi_{i1})_B + E(\pi_{i2})_B = \frac{W}{2} - 2 \cdot \overline{K};$$
$$V(\pi_i)_B = V(\pi_{i1})_B + V(\pi_{i2})_B = \frac{W^2}{8}.$$

Under the globally optimal policy, the expected value of of agent i's monetary payoff from both periods for $e_{it} = e_{jt} = \overline{e}$ is

$$E(\pi_i)_G = 0.5 \cdot W_2 - \overline{K} - \overline{K} = \frac{W}{2} - 2 \cdot \overline{K},$$

and its variance

$$V(\pi_i)_G = 0.5 \cdot \left(W_2 - \overline{K} - \overline{K} - E(\pi_i)_G\right)^2 + 0.5 \cdot \left(-\overline{K} - \overline{K} - E(\pi_i)_G\right)^2 = \frac{W^2}{4}.$$

We see that $E(\pi_i)_B = E(\pi_i)_G$, while $V(\pi_i)_B < V(\pi_i)_G$. This means that for $e_{it} = e_{jt} = \overline{e}$, the expected monetary payoffs of agent i under the baseline policy and under the globally optimal policy are equal. However, the variance of the payoff is lower under the baseline policy. This means that for $e_{it} = e_{jt} = \overline{e}$, the expected utility of a risk-averse agent is lower under the globally optimal policy than under the baseline policy. Therefore, under the globally optimal policy, it is more likely that the agent prefers to cut back on effort by exerting $e_{i1} = e_{i2} = 0$ instead of $e_{i1} = e_{i2} = \overline{e}$ than it is under the baseline policy. This implies that a risk-averse subject may have lower incentives to exert a given effort level under the baseline policy than under the globally optimal policy.

[80] To understand the result for the total variance, note that the covariance between first- and second-period payoff is 0 if the agents choose first-and second-period efforts simultaneously.

A2.6 Instructions

This section contains the instructions for session 1. The instructions for session 2 and 3 were analogous.

Instructions

General Information

Three parts, 10 periods each

This is experiment has 3 parts (Part I, Part II, and Part III). Each part is divided into 10 periods. Thus, there are 30 periods in total.

The instructions on this page and on pages 2 and 3 are relevant for all three parts. Instructions which are specific to Part I will follow on page 4. Instructions which are specific to Part II and Part III will be distributed to you before the corresponding part.

In each period, you will generate a payoff. The payoff depends on your decisions in that period and the decisions of others in that period. How you generate a payoff will be explained to you in the following.

Final payoff

Your final payoff from the experiment will be a participation payment of 10 CHF plus the payoff you generated in one randomly chosen period. The period that is randomly chosen will be the same period for all participants. Every period is equally likely to be chosen. During the experiment, you will not know which period will be chosen. Therefore, you should treat each period as if it would be the one that is relevant for your final payoff.

Final payoff = 10 CHF + your payoff from a randomly chosen period

Upon completion of the experiment, you will be paid individually and in private.

Exchange rate

Throughout the experiment, payoffs are expressed in terms of "points". At the end of the experiment, payoffs in points will be converted into payoffs in CHF. The exchange rate is:

10 points = 1 CHF

Rules

If you have any questions during the experiment, please raise your hand and wait for an experimenter to come to you. During the experiment, you are not allowed to communicate with other participants, exclaim, use personal electronic devices, or use the computer in a way not specified by the experimenter. If you are not following these rules, you may be excluded from the experiment and might only receive the participation payment.

What happens in a period

Interaction with randomly chosen participant

In each of the 30 periods, every participant is assigned into a pair with one randomly chosen other participant. In the following, we will refer to the other randomly chosen participant as the "*other*". The participants will never know the identity of the other, nor will the other know their identity.

Instructions – Page 2/5

Two stages per period Each period consists of two stages. In each stage, the participants must make a decision. In the following, this is explained in further detail.

Stage 1 works in the following way:

Stage 1: input First, each participant individually chooses an input level between 0 and 55 in increments of 0.5. The numbers are entered in the corresponding field of the computer screen. Choosing a positive input level is costly for the participants. A detailed explanation of the costs the participants have to pay for choosing a particular input level follows below.

Stage 1: output Second, the computer determines each participant's output level in Stage 1. A participant's output level depends on the participant's input level in Stage 1 and on a random number. This random number is drawn for each participant individually in Stage 1.

output level in Stage 1 = input level in Stage 1
+ random number in Stage 1

On average, the random numbers are zero, but they can take up positive and negative values. Positive and negative values are equally likely, and values close to zero are more likely than values further away from zero. On page 2 of the appendix, you find a detailed explanation of the distribution of the random numbers.

This means that on average, a participant's output level corresponds exactly to this participant's input level. Thus, a higher input level results on average in a higher output level. However, depending on the realization of the random number (positive or negative), the output level can positively or negatively deviate from the chosen input level. Positive and negative deviations are equally likely, and small deviations are more likely than large deviations.

Stage 1: information At the end of Stage 1, the computer screen displays the following information to each participant: the participant's own output level in Stage 1, the other's output level in Stage 1, and the difference between the participant's own output level and the other's output level. Note that the participants will not know the other's input level, nor will the other know their input level.

Stage 2 Stage 2 works in the same way: Each participant chooses an input level. Choosing a positive input level is costly to the participants. Then, the computer draws another random number for each participant and determines each participant's output level in Stage 2, which is the sum of the participant's input level in Stage 2 and the participant's random number in Stage 2. Finally, the computer displays the corresponding information.

Instructions – Page 3/5

Payments At the end of each period, the participants receive payments depending on their own and the other's output in Stage 1 and in Stage 2. The rules according to which these payments are made differ between the three parts of the experiment. The payment rules in each part will be explained in the instructions specific to this part, i.e., the payment rules for Part I are explained on page 4.

Payoff A participant's payoff is calculated in the same way in all three parts: It is equal to the difference between the participant's total payments and the costs for the participant's inputs in both stages, plus a fixed payment of 200 points.

payoff from a period = total payments
- costs for input in Stage 1
- costs for input in Stage 2
+ 200

A participant's payoff is therefore higher when the participant's payments are higher and the participant's costs for the inputs are lower. Note that a participant's payoff will never be negative.

Costs On page 1 of the appendix, you find a table and a graph showing which costs the participants have to pay for choosing a particular input level in a stage. The costs are increasing in the input level chosen by a participant. That is, the higher is the chosen input level, the higher are the costs the participant has to pay.

No predictions possible Note that it is not possible to make predictions about future draws of random numbers from draws of random numbers in the past. The random numbers are newly drawn for every participant in every stage of every period, and these draws are independent from each other.

Instructions – Page 4/5

Instructions specific to Part I

In Part I, the **payment rules** after Stage I and Stage II are the following:

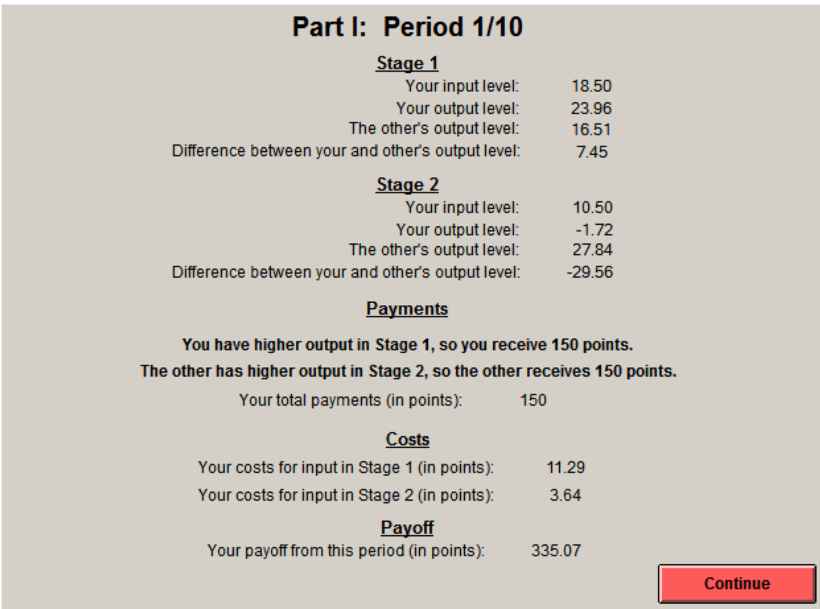
- A first payment of 150 points is given to the participant in the pair who has higher output in Stage 1. The participant with lower output in Stage 1 receives no payment. If both outputs in Stage 1 are equal, the participant who receives the first payment is randomly chosen.
- A second payment of 150 points is given to the participant in the pair who has higher output in Stage 2. The participant with lower output in Stage 2 receives no payment. If both outputs in Stage 2 are equal, the participant who receives the second payment is randomly chosen.

150 points to participant with higher output in Stage 1
150 points to participant with higher output in Stage 2

This means that output in Stage 1 only counts for the first payment, and output in Stage 2 only counts for the second payment.

Example – Part I

In the following, you take up the perspective of some participant in one of the 10 periods of Part I. Below, you see a picture of the computer screen after Stage 1 and Stage 2. Note that the numbers only serve as an example to illustrate the rules of Part I, and are not a recommendation towards what you should do.



Instructions – Page 5/5

In **Stage 1**, you chose an input level of 18.5. The computer then determined your output level in Stage 1 as 23.96. This means that your random number was +5.46 (23.96 + 5.46 = 18.5). The other's output level in Stage 1 was 16.51. The computer thus displays the difference between your and the other's output level as 7.45 (23.96 - 16.51 = 7.45).

In **Stage 2**, you chose an input level of 10.50. The computer then determined your output level in Stage 2 as -1.72. This means that your random number was -12.22 (10.5 - 12.22 = -1.72). The other's output level in Stage 2 was 27.84. The computer thus displays the difference between your and the other's output level as -29.56 (-1.72 - 27.84 = -29.56).

Then, you and the other received **payments** depending on your and the other's output in Stage 1 and in Stage 2. First, since your output in Stage 1 (23.96) was higher than the other's output in Stage 1 (16.51), a payment of 150 points was given to you. Second, since your output in Stage 2 (-1.72) was lower than the other's output in Stage 2 (27.84), a payment of 150 points was given to the other. Thus, your total payments were 150 points (150 + 0 = 150).

The **costs** for your input level of 18.5 in Stage 1 were 11.29 points, and the costs for your input level of 10.5 in Stage 2 were 3.64 points.

Your **payoff from this period** are your total payments (150 points) minus your costs for input in Stage 1 (11.29 points) and for input in Stage 2 (3.64 points), plus the fixed payment of 200 points. Your payoff from this period is thus 335.07 points (150 - 11.29 - 3.64 + 200 = 335.07).

Suppose this period would be randomly chosen to be relevant for your **final payoff**. Your final payoff would then be 45.51 CHF, which is the sum of the 10 CHF participation payment and the payoff from this period converted into 33.51 CHF (= 355.07/10).

Instructions specific to Part II

In Part II, the **payment rules** after Stage I and Stage II are the following:

- A first payment of 150 points is given to the participant in the pair who has higher output in Stage 1. The participant with lower output in Stage 1 receives no payment. If both outputs in Stage 1 are equal, the participant who receives the first payment is randomly chosen.
- A second payment of 150 points is given to the participant in the pair whose sum of output in Stage 1 and output in Stage 2 is higher. The participant whose sum of output in Stage 1 and output in Stage 2 is lower receives no payment. If both participants have the same sum of output in Stage 1 and output in Stage 2, the participant who receives the second payment is randomly chosen.

150 points to participant with higher output in Stage 1
150 points to participant with higher sum of output

This means that output in Stage 1 counts both for the first payment and for the second payment, while output in Stage 2 only counts for the second payment.

Example – Part II

Reconsider the example from Part I. You had an output level of 23.96 in Stage 1 and of -1.72 in Stage 2. The sum of your output in Stage 1 and output in Stage 2 is then 22.24 (23.96 + (-1.72) = 22.24).

The other had an output level of 16.51 in Stage 1 and of 27.84 in Stage 2. The other's sum of output in Stage 1 and output in Stage 2 is then 44.35 (16.51 + 27.84 = 44.35).

With the rules of Part II, payments are as follows:

First, since your output in Stage 1 (23.96) is higher than the other's output in Stage 1 (16.51), a payment of 150 points is given to you. Second, since your sum of output (22.24) is smaller than the other's sum of output (44.35), a payment of 150 points is given to the other. Your total payments are thus 150 points (150 + 0 = 150).

Suppose you chose the same input levels as in the example for Part I. Your costs for input are thus 11.29 for Stage 1, and 3.64 for Stage 2. Your payoff from this period is thus 335.07 points (150 - 11.29 - 3.64 + 200 = 335.07).

Additional Instructions (II) – Page 1/1

Instructions specific to Part III

In Part III, the **payment rules** after Stage I and Stage II are the following:

- A payment of 300 points is given to the participant whose sum of output in Stage 1 and output in Stage 2 is higher. The participant whose sum of output in Stage 1 and output in Stage 2 is lower receives no payment. If both participants have the same sum of output in Stage 1 and output in Stage 2, the participant who receives the second payment is randomly chosen.

300 points to participant with higher sum of output

This means that output in Stage 1 and output in Stage 2 both count for the payment.

Example – Part III

Reconsider the example from Part I. You had an output level of 23.96 in Stage 1 and of -1.72 in Stage 2. The sum of your output in Stage 1 and output in Stage 2 is then 22.24 (23.96 + (-1.72) = 22.24).

The other had an output level of 16.51 in Stage 1 and of 27.84 in Stage 2. The other's sum of output in Stage 1 and output in Stage 2 is then 44.35 (16.51 + 27.84 = 44.35).

With the rules of Part III, payments are as follows:

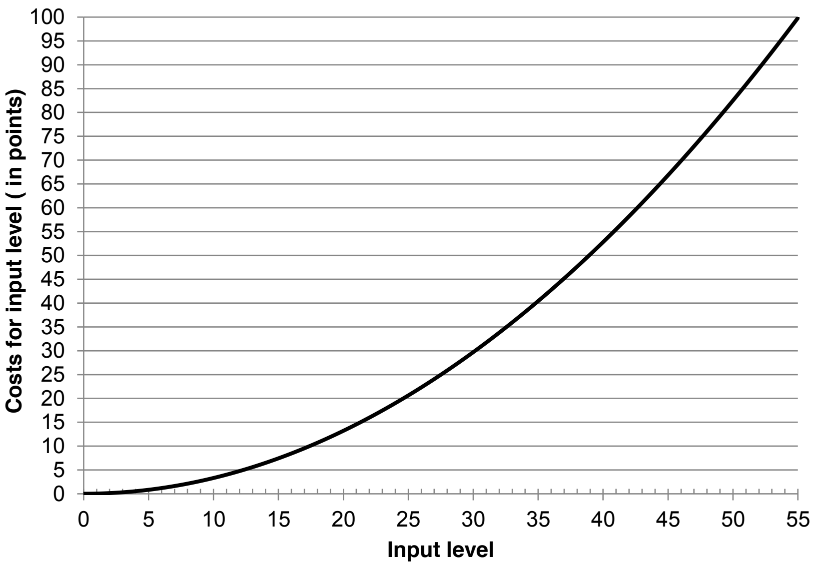
Since your sum of output (22.24) is smaller than the other's sum of output (44.35), a payment of 300 points is given to the other. Your total payments are thus 0 points (0 + 0 = 0).

Suppose you chose the same input levels as in the example for Part I. Your costs for input are thus 11.29 for Stage 1, and 3.64 for Stage 2. Your payoff from this period is thus 185.07 points (0 - 11.29 - 3.64 + 200 = 185.07).

Appendix – Page 1/2

Costs for input level depending on choice of input level

input level	costs for input level (in points)	input level	costs for input level (in points)	input level	costs for input level (in points)	input level	costs for input level (in points)
0.0	0.00	14.0	6.47	28.0	25.87	42.0	58.21
0.5	0.01	14.5	6.94	28.5	26.80	42.5	59.61
1.0	0.03	15.0	7.43	29.0	27.75	43.0	61.02
1.5	0.07	15.5	7.93	29.5	28.72	43.5	62.44
2.0	0.13	16.0	8.45	30.0	29.70	44.0	63.89
2.5	0.21	16.5	8.98	30.5	30.70	44.5	65.35
3.0	0.30	17.0	9.54	31.0	31.71	45.0	66.83
3.5	0.40	17.5	10.11	31.5	32.74	45.5	68.32
4.0	0.53	18.0	10.69	32.0	33.79	46.0	69.83
4.5	0.67	18.5	11.29	32.5	34.86	46.5	71.35
5.0	0.83	19.0	11.91	33.0	35.94	47.0	72.90
5.5	1.00	19.5	12.55	33.5	37.03	47.5	74.46
6.0	1.19	20.0	13.20	34.0	38.15	48.0	76.03
6.5	1.39	20.5	13.87	34.5	39.28	48.5	77.62
7.0	1.62	21.0	14.55	35.0	40.43	49.0	79.23
7.5	1.86	21.5	15.25	35.5	41.59	49.5	80.86
8.0	2.11	22.0	15.97	36.0	42.77	50.0	82.50
8.5	2.38	22.5	16.71	36.5	43.96	50.5	84.16
9.0	2.67	23.0	17.46	37.0	45.18	51.0	85.83
9.5	2.98	23.5	18.22	37.5	46.41	51.5	87.52
10.0	3.30	24.0	19.01	38.0	47.65	52.0	89.23
10.5	3.64	24.5	19.81	38.5	48.91	52.5	90.96
11.0	3.99	25.0	20.63	39.0	50.19	53.0	92.70
11.5	4.36	25.5	21.46	39.5	51.49	53.5	94.45
12.0	4.75	26.0	22.31	40.0	52.80	54.0	96.23
12.5	5.16	26.5	23.17	40.5	54.13	54.5	98.02
13.0	5.58	27.0	24.06	41.0	55.47	55.0	99.83
13.5	6.01	27.5	24.96	41.5	56.83		

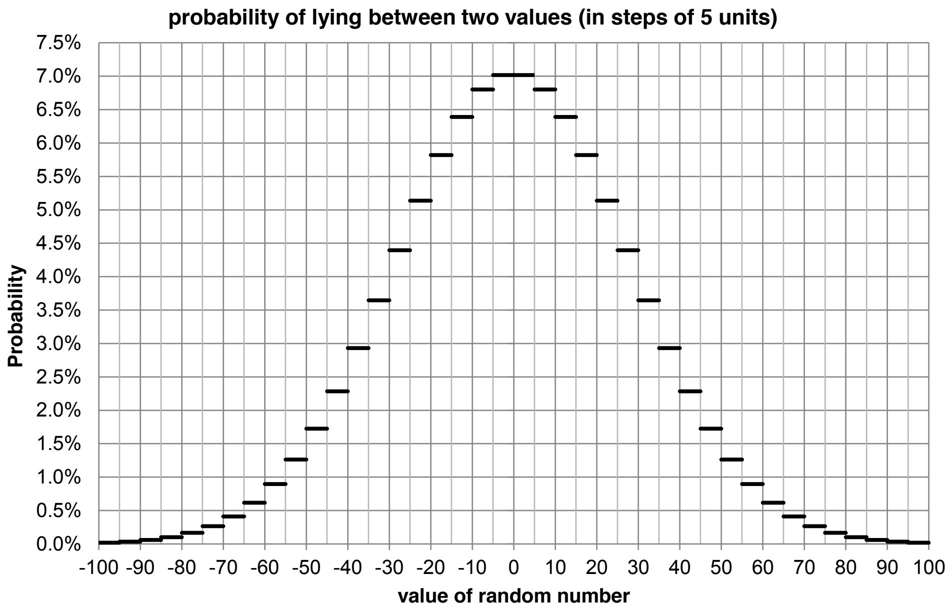


Appendix – Page 2/2

Distribution of the random numbers

In every stage of every period, a participant's random number is drawn from a normal distribution with expected value 0 and standard deviation 28.28.

The following graph shows the probabilities that the random number lies between two values (in steps of 5 units):



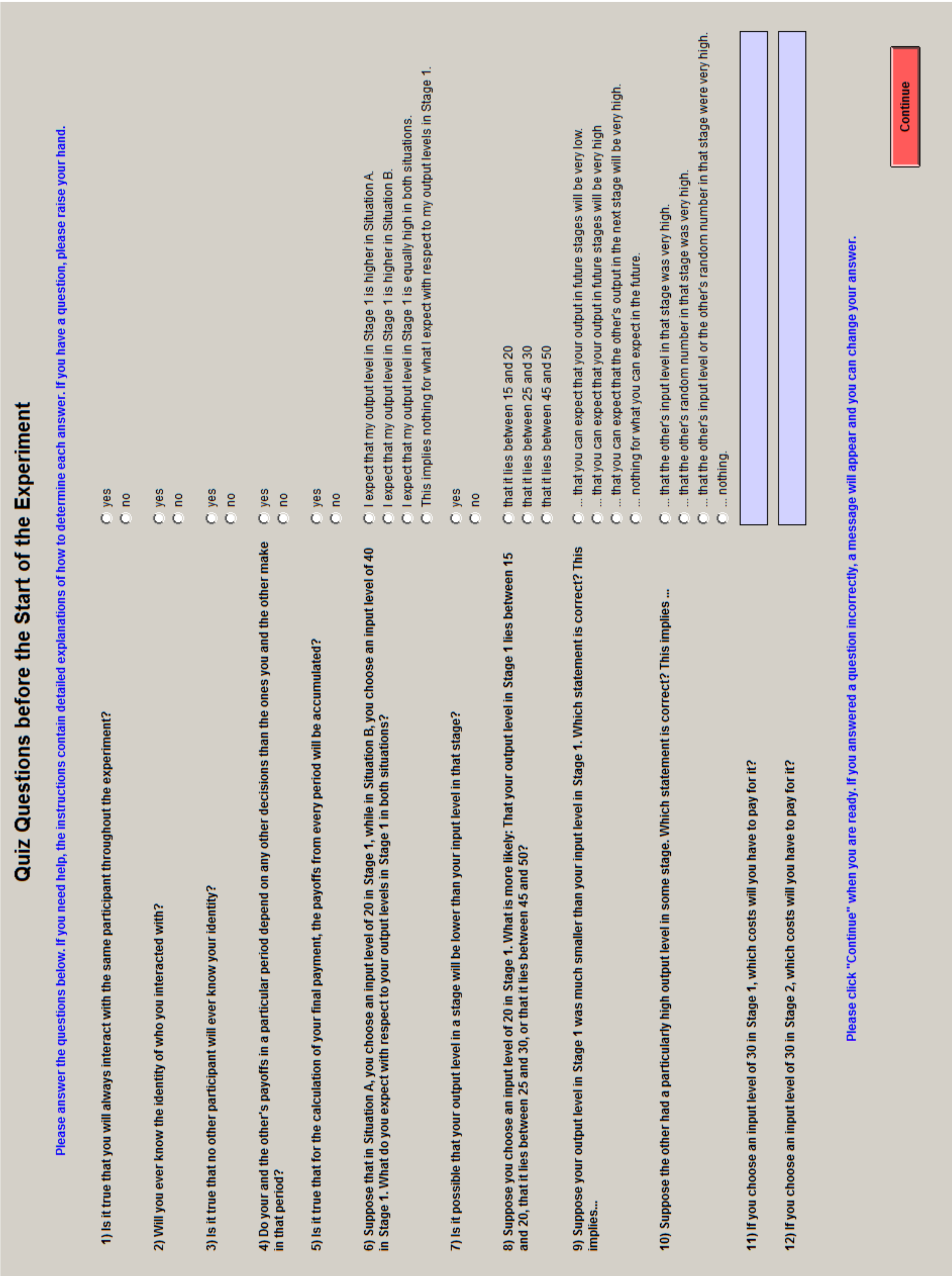
How to read the graph: The horizontal bars each represent a certain range of possible values for the random number. On the horizontal axis, you can read the left and the right boundary of a range. On the vertical axis, you can read the probability that the random number lies within this range.

Example: The probability that the random number lies between +5 and +10 is about 6.8%. This is equal to the probability that the random number lies between -10 and -5. This means that if you choose an input level of, say, 20, the probability that your output level lies between 25 and 30 is about 6.8%, and the probability that your output level lies between 10 and 15 is also 6.8%.

Note that by adding up the probabilities of the four bars to the right of zero (7, 6.8, 6.4, 5.8) and of the four bars to the left of zero (5.8, 6.4, 6.8, 7), you learn that the probability that the output level is within +/- 20 units around your input level is about 52%.

A2.7 Control questions

This section contains the control questions that the participants had to solve at the beginning of the experiment and before treatment BASE (here shown for session 1). The control questions for treatments PART and GLOB were analogous to those for BASE.



Quiz Questions specific to Part I

Please answer the questions below. If you need help, the instructions contain detailed explanations of how to determine each answer. If you have a question, please raise your hand.

Suppose you are in some period of Part I.

1) With the input level you choose in Stage 1, you can affect...

- ... only who gets the first payment of 150 points.
- ... only who gets the second payment of 150 points.
- ... who gets the first payment of 150 points AND who gets the second payment of 150 points.
- ... nothing.

2) With the input level you choose in Stage 2, you can affect...

- ... only who gets the first payment of 150 points.
- ... only who gets the second payment of 150 points.
- ... who gets the first payment of 150 points AND who gets the second payment of 150 points.
- ... nothing.

3) Is it true that if your output in Stage 1 is much lower than the other's output in Stage 1, your output in Stage 2 must necessarily be much higher than the other's output in Stage 2 to receive the second payment of 150 points?

- yes
- no

Suppose your output level in Stage 1 is 10, and your output level in Stage 2 is 20. Suppose the other's output level in Stage 1 is -5, and the other's output level in Stage 2 is 45.

4) What are your total payments in this period (in points)?

5) What the other's total payments in this period (in points)?

Please click "Continue" when you are ready. If you answered a question incorrectly, a message will appear and you can change your answer.

Continue

A2.8 Computer interface


Part I: Period 1/10
Stage 1
Your input level: 40.00
Your output level: 7.50
The other's output level: 35.44
Difference between your and other's output level: -27.94
Stage 2
Your input level in Stage 2:
To Do
Please make your input choice for Stage 2.
When you are ready, click "Continue" to proceed to the results of Stage 2 and of the whole period.
Continue


remaining time [sec]: 36
Part I: Period 1/10
To Do
When you are ready, click "Continue" to finish this period.
Stage 1
Your input level: 40.00
Your output level: 7.50
The other's output level: 35.44
Difference between your and other's output level: -27.94
Stage 2
Your input level: 35.00
Your output level: 26.44
The other's output level: 11.62
Difference between your and other's output level: 14.82
Payments
The other has higher output in Stage 1, so the other receives 150 points.
You have higher output in Stage 2, so you receive 150 points.
Your total payments (in points): 150
Costs
Your costs for input in Stage 1 (in points): 52.80
Your costs for input in Stage 2 (in points): 40.42
Payoff
Your payoff from this period (in points): 256.78
Continue

References

Anderson, L. R. & Freeborn, B. A. (2010). Varying the intensity of competition in a multiple prize rent seeking experiment. *Public Choice*, 143(1-2), 237–254.

Aoyagi, M. (2010). Information feedback in a dynamic tournament. *Games and Economic Behavior*, 70(2), 242–260.

Balafoutas, L., Kerschbamer, R., & Sutter, M. (2012). Distributional preferences and competitive behavior. *Journal of Economic Behavior & Organization*, 83(1), 125–135.

Bartling, B., Fehr, E., Maréchal, M. A., & Schunk, D. (2009). Egalitarianism and competitiveness. *The American Economic Review (Papers and Proceedings)*, 99(2), 93–98.

Bock, O., Nicklisch, A., & Baetge, I. (2012). hroot: Hamburg registration and organization online tool. WiSo-HH Working Paper No. 1.

Brookins, P. & Ryvkin, D. (2014). An experimental study of bidding in contests of incomplete information. *Experimental Economics*, 17(2), 245–261.

Brown, J. (2011). Quitters never win: The (adverse) incentive effects of competing with superstars. *Journal of Political Economy*, 119(5), 982–1013.

Calaway, R. & Weston, S. (2014a). doMC: Foreach parallel adaptor for 'parallel'. R package version 1.3.3, http://CRAN.R-project.org/package=doMC.

Calaway, R. & Weston, S. (2014b). foreach: Provides foreach looping construct for R. R package version 1.4.2, http://CRAN.R-project.org/package=foreach.

Cameron, A. C., Gelbach, J. B., & Miller, D. L. (2008). Bootstrap-based improvements for inference with clustered errors. *The Review of Economics and Statistics*, 90(3), 414–427.

Chen, H., Ham, S. H., & Lim, N. (2011). Designing multiperson tournaments with asymmetric contestants: An experimental study. *Management Science*, 57(5), 864–883.

Chlosta, K., Pull, K., & Futagami, S. (2014). Tournament structures in Japan and the U.S.: Why are they different and will they change? *Management and Organizational Studies*, 1(1), 63–71.

Cubitt, R. P., Starmer, C., & Sugden, R. (1998). On the validity of the random lottery incentive system. *Experimental Economics*, 1(2), 115–131.

Datta, S. & Satten, G. A. (2008). A signed-rank test for clustered data. *Biometrics*, 64(2), 501–507.

Dechenaux, E., Kovenock, D., & Sheremeta, R. M. (2012). A survey of experimental research on contests, all-pay auctions and tournaments. WZB Discussion Paper No. SP II 2012-109.

Delfgaauw, J., Dur, R., Non, A., & Verbeke, W. (2012). The effects of prize spread and noise in elimination tournaments: A natural field experiment. IZA Discussion Paper No. 6480.

Dohmen, T., Falk, A., Huffman, D., Sunde, U., Schupp, J., & Wagner, G. G. (2011). Individual risk attitudes: Measurement, determinants, and behavioral consequences. *Journal of the European Economic Association*, 9(3), 522–550.

Drago, R. & Heywood, J. S. (1991). Subject response to loss in an experimental tournament. *Economics Letters*, 36(4), 371–374.

Ederer, F. (2010). Feedback and motivation in dynamic tournaments. *Journal of Economics & Management Strategy*, 19(3), 733–769.

Ederer, F. & Fehr, E. (2007). Deception and incentives: How dishonesty undermines effort provision. IZA Discussion Paper No. 3200.

Fehr, E. & Williams, T. (2013). Endogenous emergence of institutions to sustain cooperation. Unpublished working paper.

Fischbacher, U. (2007). z-tree: Zurich toolbox for ready-made economic experiments. *Experimental Economics*, 10(2), 171–178.

Fonseca, M. A. (2009). An experimental investigation of asymmetric contests. *International Journal of Industrial Organization*, 27(5), 582–591.

Freeman, R. B. & Gelber, A. M. (2010). Prize structure and information in tournaments: Experimental evidence. *American Economic Journal: Applied Economics*, 2(1), 149–164.

Gächter, S., Johnson, E. J., & Herrmann, A. (2010). Individual-level loss aversion in riskless and risky choices. CeDEx Discussion Paper No. 2010-20.

Gaujoux, R. (2014). doRNG: Generic reproducible parallel backend for foreach loops. R package version 1.6, http://CRAN.R-project.org/package=doRNG.

Gershkov, A. & Perry, M. (2009). Tournaments with midterm reviews. *Games and Economic Behavior*, 66(1), 162–190.

Harbaugh, R. & Ridlon, R. W. (2010). Handicapping under uncertainty in an all-pay auction. Working paper. Retrieved 2015/01/15, from http://ssrn.com/abstract=1688160.

Harbring, C. & Irlenbusch, B. (2003). An experimental study on tournament design. *Labour Economics*, 10(4), 443–464.

Harbring, C. & Lünser, G. K. (2008). On the competition of asymmetric agents. *German Economic Review*, 9(3), 373–395.

Harris, P. B. & Houston, J. M. (2010). A reliability analysis of the revised competitiveness index. *Psychological Reports*, 106(3), 870–874.

Henningsen, A. (2013). censReg: Censored regression (Tobit) models. R package version 0.5-20, http://CRAN.R-project.org/package=censReg.

Hernandez, P., Minor, D., & Sisak, D. (2013). Performance and relative incentive pay: The role of social preferences. Tinbergen Institute Discussion Paper 2013-176/VII.

Houston, J., Harris, P., McIntire, S., & Francis, D. (2002). Revising the competitiveness index using factor analysis. *Psychological Reports*, 90(1), 31–34.

Højsgaard, S. & Halekoh, U. (2012). doBy: Groupwise statistics, LSmeans, linear contrasts, utilities. R package version 4.5.2, http://CRAN.R-project.org/package=doBy.

Jönsson, S. (2013). Implementation and optimality of all-pay auctions: Experimental evidence. Unpublished working paper.

King, G. & Roberts, M. E. (2014). How robust standard errors expose methodological problems they do not fix, and what to do about it. *Political Analysis*, (pp. 1–21).

Kleiber, C. & Zeileis, A. (2008). *Applied Econometrics with R*. New York: Springer-Verlag.

Klein, A. H. & Schmutzler, A. (2014). Optimal effort incentives in dynamic tournaments. CEPR Discussion Paper No. 10192.

Kong, X. (2008). Loss aversion and rent-seeking: An experimental study. CeDEx Discussion Paper No. 2008-13.

Letina, I. & Schmutzler, A. (2014). Procuring diversity. Unpublished working paper.

Likert, R. (1932). A technique for the measurement of attitudes. *Archives of Psychology*, 22(140), 1–55.

Lim, N., Ahearne, M. J., & Ham, S. H. (2009). Designing sales contests: Does the prize structure matter? *Journal of Marketing Research*, 46(3), 356–371.

Ludwig, S. & Lünser, G. K. (2012). Observing your competitor – The role of effort information in two-stage tournaments. *Journal of Economic Psychology*, 33(1), 166–182.

McGregor, J. (2013). For whom the bell curve tolls. *The Washington Post – On Leadership* [weblog]. Retrieved 2015/01/15, from http://www.washingtonpost.com/blogs/on-leadership/wp/2013/11/20/for-whom-the-bell-curve-tolls/.

Meyer, M. A. (1992). Biased contests and moral hazard: Implications for career profiles. *Annales d'Économie et de Statistique*, 25/26, 165–187.

Millner, E. L. & Pratt, M. D. (1991). Risk aversion and rent-seeking: An extension and some experimental evidence. *Public Choice*, 69(1), 81–92.

Morgan, J. & Wang, R. (2010). Tournaments for ideas. *California Management Review*, 52(2), 77–97.

Müller, W. & Schotter, A. (2010). Workaholics and dropouts in organizations. *Journal of the European Economic Association*, 8(4), 717–743.

Murphy, R. O., Ackermann, K. A., & Handgraaf, M. J. J. (2011). Measuring social value orientation. *Judgment and Decision Making*, 6(8), 771–781.

Orrison, A., Schotter, A., & Weigelt, K. (2004). Multiperson tournaments: An experimental examination. *Management Science*, 50(2), 268–279.

Price, C. R. & Sheremeta, R. M. (2011). Endowment effects in contests. *Economics Letters*, 111(3), 217–219.

Price, C. R. & Sheremeta, R. M. (2012). Endowment origin, demographic effects and individual preferences in contests. Working paper. Retrieved 2015/01/15, from http://ssrn.com/abstract=2030470.

R Core Team (2014). *R: A Language and Environment for Statistical Computing*. R Foundation for Statistical Computing, Vienna, Austria.

Ridlon, R. & Shin, J. (2013). Favoring the winner or loser in repeated contests. *Marketing Science*, 32(5), 768–785.

Rosett, R. N. & Nelson, F. D. (1975). Estimation of the two-limit probit regression model. *Econometrica*, 43(1), 141–146.

Schmitt, P., Shupp, R., Swope, K., & Cadigan, J. (2004). Multi-period rent-seeking contests with carryover: Theory and experimental evidence. *Economics of Governance*, 5(3), 187–211.

Schotter, A. & Weigelt, K. (1992). Asymmetric tournaments, equal opportunity laws, and affirmative action: Some experimental results. *The Quarterly Journal of Economics*, 107(2), 511–539.

Sheremeta, R. M. (2010a). Expenditures and information disclosure in two-stage political contests. *Journal of Conflict Resolution*, 54(5), 771–798.

Sheremeta, R. M. (2010b). Experimental comparison of multi-stage and one-stage contests. *Games and Economic Behavior*, 68(2), 731–747.

Sheremeta, R. M. (2011). Contest design: An experimental investigation. *Economic Inquiry*, 49(2), 573–590.

Sheremeta, R. M., Masters, W. A., & Cason, T. N. (2013). Winner-take-all and proportional-prize contests: Theory and experimental results. Working paper. Retrieved 2015/01/15, from http://ssrn.com/abstract=2014994.

Sheremeta, R. M. & Zhang, J. (2010). Can groups solve the problem of overbidding in contests? *Social Choice and Welfare*, 35(2), 175–197.

Shupp, R., Sheremeta, R. M., Schmidt, D., & Walker, J. (2013). Resource allocation contests: Experimental evidence. *Journal of Economic Psychology*, 39, 257–267.

Stracke, R., Höchtl, W., Kerschbamer, R., & Sunde, U. (2014). Optimal prizes in dynamic elimination contests: Theory and experimental evidence. *Journal of Economic Behavior & Organization*, 102, 43–58.

Tillé, Y. & Matei, A. (2013). sampling: Survey sampling. R package version 2.6, http://CRAN.R-project.org/package=sampling.

Tversky, A. & Kahneman, D. (1992). Advances in prospect theory: Cumulative representation of uncertainty. *Journal of Risk and Uncertainty*, 5(4), 297–323.

Welch, J. (2005). The vitality curve. *Leadership Excellence*, 22(9), 4–5.

White, H. (1980). A heteroskedasticity-consistent covariance matrix estimator and a direct test for heteroskedasticity. *Econometrica*, 48, 817–838.

Zeileis, A. (2004). Econometric computing with HC and HAC covariance matrix estimators. *Journal of Statistical Software*, 11(10), 1–17.

Zeileis, A. & Hothorn, T. (2002). Diagnostic checking in regression relationships. *R News*, 2(3), 7–10.

Chapter 3

A Matter of Perspective: How Fairness Views Depend on Relative Income

joint with Lea Cassar

3.1 Introduction

The redistribution of income has been and will presumably remain one of the most debated aspects of public policy in modern economies. Furthermore, many other economic policy measures that do not aim at redistribution per se, but have redistributive consequences, are subject to controversial discussions. It emerges clearly from these debates that preferences for redistribution vary significantly between individuals. What explains this variation? As emphasized by Giuliano and Spilimbergo (2014, p. 787), our knowledge on this issue is still very limited: "Despite the crucial role of preferences for redistribution in explaining institutional outcomes, little empirical work has been done on how these preferences are formed and how and why they change over time".

A well-established finding in the empirical literature is the negative relationship between preferences for redistribution and income (Fong, 2001; Alesina & La Ferrara, 2005; Alesina & Fuchs-Schündeln, 2007; Alesina & Giuliano, 2011; Luttmer & Singhal, 2011; Giuliano & Spilimbergo, 2014; Owens & Pedulla, 2014; Powdthavee & Oswald, 2014).[1] A simple explanation for this pattern is self-interest: While

[1]All of these papers use survey data. Kataria and Montinari (2012) and Durante et al. (2014) provide experimental evidence for a negative relationship between income and tax choices. Agranov

high-income individuals want less redistribution to avoid high taxes, low-income individuals want to benefit from transfers and thus support more redistribution.

In this study, we argue that self-interest might not represent the full story. Empirical evidence suggests that preferences for redistribution are not only driven by self-interest, but also by individuals' views on what is a fair distribution of income, henceforth *fairness views* (Cappelen et al., 2007; Almås et al., 2010; Cappelen et al., 2010, 2013). In our experiment, we show that relative income has a causal effect on these fairness views. This suggests that income affects individuals' preferences for redistribution for reasons that go beyond the self-interest channel – because it also changes their view on what is a fair income distribution.

The studies revealing a negative relationship between preferences for redistribution and income cannot tell whether this is due to a causal effect of income on fairness views, and, therefore, whether the observed negative relationship goes beyond reasons of self-interest. This lack of evidence can be easily attributed to the endogeneity of income and/or to the difficulty of disentangling the effect of fairness views from selfish motives when eliciting preferences for redistribution from field data.[2] Experiments represent, therefore, a useful complementary tool to address both of these issues.[3]

A series of experimental papers focuses on understanding the different types of fairness views present in society (Frohlich et al., 2004; Cappelen et al., 2007; Almås et al., 2010; Cappelen et al., 2010, 2013; Möllerström et al., 2014). These papers, however, treat fairness views as exogenous and do not investigate how they

---

and Palfrey (2014) obtain an analogous result for the relationship between productivity and tax choices.

[2] Even when income is exogenously generated, as in Owens and Pedulla (2014) and Powdthavee and Oswald (2014), individuals who experience a positive income shock may report a lower preference for redistribution because of selfish motives, i.e., to reduce the tax burden at their new position in the income distribution, and/or because their fairness views become less egalitarian. In this latter case, but not necessarily in the former, individuals would vote for low redistribution even when they have no private interests at stake. This difference in motives, however, is hardly observable in field data.

[3] A related strand of literature focuses on the relationship between *giving* and income (Buckley & Croson, 2006; James & Sharpe, 2007; Piff et al., 2010; Erkal et al., 2011). While these papers obtain mixed results, they cannot clarify the relationship between fairness views and income either. On the one hand, this is due to the challenge of inferring fairness views from giving behavior, for which it is necessary to have variation in the way how income differences between givers and receivers were generated, as well as to disentangle selfish motives from fairness motives, e.g. through the estimation of a structural model as introduced by Cappelen et al. (2007). The papers focusing on the relationship between income and giving satisfy neither of these two requirements. Note, however, that the results of Erkal et al. (2011), who find in their experiment that high-income individuals give less than low-income individuals, at least suggest that fairness views of the participants differ across incomes. A further reason why the studies on the relationship between income and giving cannot clarify a causal effect of income on fairness views is the endogeneity of income in these studies. However, in the experiment of Buckley and Croson (2006) and in one treatment of Erkal et al. (2011), income is exogenous, but has no effect on giving.

are formed or why they may vary among individuals. The determinants of these fairness views, and in particular the potential role of income, remain, therefore, largely unknown.[4,5]

This study reports the results from a laboratory experiment that was designed to investigate (1) if individuals' relative income affects their fairness views and (2) whether this effect depends on how the income is generated. The experiment consisted of an income generation phase and a distribution phase. In the income generation phase, participants received a high or a low income either through luck or through an effort-based tournament. In the distribution phase, we asked a subset of subjects to make distributive decisions over the incomes of two other pairs of subjects – one pair in which income differences were due to luck, and one in which income differences were due to effort. Thus, in contrast to previous studies, the distributors in our experiment were neither stakeholders, as they had no monetary stakes when making the decisions, nor were they impartial spectators, as they had participated in the income generation phase. This novel design enabled us to test how the distributors' outcomes in the income generation phase – not only the relative income but also whether the latter was generated by luck or by effort – affected their fairness views. Furthermore, we elicited fairness views about the distribution of income from luck and about the distribution of income from effort.

Our results can be summarized as follows. First, we find that low-income individuals redistribute significantly more than high-income individuals when the source of income differences is the same as the one they experienced themselves. That is, when inequalities are due to luck (effort), an individual who received a low income by lack of luck (effort) redistributes significantly more than an individual who received a high income by luck (effort). The effect remains unchanged when controlling for individual performance in the effort-based tournament, suggesting that self-selection into different income levels does not drive the results. We thus conclude that relative income, and how the latter is generated, has a causal effect on individuals' fairness views, and therefore, on preferences for redistribution beyond the self-interest channel.

Second, subjects' responses in a post-experimental questionnaire suggest that the effect of income on redistributive choices comes along with a consistent shift in beliefs about the degree of individuals' responsibility for a specific outcome. More

[4] Other studies try to investigate whether fairness principles may be chosen in an opportunistic way, i.e., to maximize one's own monetary payoff (Rodriguez-Lara & Moreno-Garrido, 2012; Ubeda, 2014; Tokumaru, 2014). In a related study, Becchetti et al. (2011) analyze how the choice of a distribution criterion depends on the knowledge of one's position in the income distribution. In our experiment, however, since participants had no monetary stakes when making their redistributive decisions, such opportunistic behavior was ruled out by design.

[5] Barr et al. (2012) relate the participants' social status to their behavior in a dictator game. They find that high-status individuals tend to acknowledge entitlement owing to effort more than low-status individuals. They cannot, however, show that this is due to a causal effect of social status, as they cannot control for self-selection.

specifically, individuals who received a high income by effort (luck) attribute outcomes in the tournament (lottery) more to internal factors (i.e., factors under their control) compared to individuals who received a low income by lack of effort (luck). One interpretation of these findings is that the effect of relative income on fairness views results from a change of beliefs about one's responsibility over an outcome. This belief follows a self-serving bias in responsibility attribution, that is, it takes credit for personal successes and denies responsibility for failures (Miller & Ross, 1975; Bradley, 1978).

This study provides two main contributions to the literature. First of all, we enrich the literature on the determinants of preferences for redistribution. We show that relative income affects preferences for redistribution through its effect on views about what is a fair distribution of income. Second, we contribute to the growing literature on fairness views. We show that people's fairness views are not fixed, but endogenous to the process of income generation, as they depend on people's relative income. Furthermore, our results on the self-serving bias in responsibility attribution emphasize the importance of beliefs for fairness views.

The study also has important implications for our understanding of how societies think about redistribution. Our results suggest that personal income changes individuals' views about a fair distribution of income in society. This implies that the conflict between rich and poor in the debate about redistribution is not only a battle of personal interests, but also of different ideologies. This difference in ideologies is such that it increases the discrepancy in preferences for redistribution that is already caused by selfish motives. This means that there will be disagreement between rich and poor about income redistribution even if people are able to abstract from their own personal stake in this redistribution. Our results further imply that the differences in ideologies between rich and poor are, at least to some degree, the result of different individual outcomes in the process of income generation. As a consequence, an increase in income inequality is likely to boost the polarization in political preferences, making it harder for societies to reach a consensus about the optimal level of redistribution in the long run.

In the following, we describe the design, experimental procedures and identification strategies in greater detail (Section 3.2), present the results (Section 3.3), discuss potential channels underlying the effects (Section 3.4), and conclude (Section 3.5).

3.2 The experiment

3.2.1 Design

The experiment consisted of two phases: An *income generation phase* and a *distribution phase*. At the beginning of the experiment, we instructed the participants only about the income generation phase, while telling them that the second phase would concern the distribution of the incomes generated in the first phase. After the income generation phase had been completed, we explained details of the distribution phase. We describe the two phases below.

3.2.1.1 Income generation phase

At the beginning of the income generation phase, the participants were randomly paired. Next, they executed a real effort task. We used a variant of the slider task introduced by Gill and Prowse (2012). This computerized task consists of a screen containing 48 sliders, which are initially positioned at zero and can be moved as far as 100 using the mouse cursor (see instructions on page 141).[6] The goal is to set as many sliders as possible to exactly fifty within 120 seconds. In our experiment, we confronted the participants with a series of five of these screens, each for 120 seconds. The total number of sliders adjusted to exactly fifty in the five screens represented the participants' *effort* in the task. Before the sequence of five screens started, the participants had 60 seconds to practice the task. After the time was up, the participants saw their effort on the computer screen.

After all participants had completed the task and had seen their effort, every pair of participants was randomly assigned to one of two treatments – a lottery treatment and a tournament treatment. More specifically, half of the pairs within a session was assigned to the tournament treatment, while the other half was assigned to the lottery treatment.

Each treatment implied a different income generation process in assigning a high and a low income within a pair. In the tournament treatment, a high income was assigned to the participant in the pair with higher effort in the task, and a low income to the other participant. In the lottery treatment, the two incomes were randomly assigned within the pair. The income levels were constant across both treatments. We paid 25 Swiss Francs (CHF) as high income and CHF 5 as low income.[7] At the end of the income generation phase, the participants observed their own income, the income of the participant they were paired with, and the process that had generated the incomes within their pair. They did, however, not observe the efforts of the other participants in the tournament.

[6]We deactivated the mouse wheels and keyboards by software, so that the participants could only use the mouse cursor to manipulate the sliders.

[7]At the time of the experiment, the exchange rate was CHF 1.22 per € and CHF 0.89 per US$.

The income generation phase thus produced four types of participants (see Table 3.1): Those with high income from the lottery (HiLot), those with low income from the lottery (LoLot), those with high income from the tournament (HiTour), and those with low income from the tournament (LoTour).

Table 3.1: Types in the income generation phase

	Lottery	Tournament
High income (CHF 25)	HiLot	HiTour
Low income (CHF 5)	LoLot	LoTour

3.2.1.2 Distribution phase

In the subsequent distribution phase, each pair was randomly assigned to one of two roles – *distributors*, who kept their income from the first phase, and *non-distributors*, whose incomes were subject to redistribution by the distributors. Per session, there were two pairs of non-distributors – one from those pairs whose incomes had been generated through the lottery, and one from those pairs whose incomes had been generated through the tournament.

In the next step, the distributors were asked to distribute the total income that was earned within each non-distributor pair between both members of that pair. This means that every distributor made two distributive decisions: One for the pair from the lottery treatment, and one for the pair from the tournament treatment.[8] The order of presentation was random, but it was always made very clear for which pair the decision was currently made. Before confirming the redistributive choices, the distributors had the possibility to go back and change the choices for both pairs if they wanted to. The decisions were such that for each pair, the distributors had to enter how much of the total income (CHF 30) should be distributed to the participant who had earned CHF 25, and how much of it should be distributed to the one who had earned CHF 5. The amounts given to both participants had to sum up to CHF 30 and were entered in multiples of CHF 0.5.[9] At the end of the distribution phase, the decisions of one distributor were randomly chosen and applied to the non-distributor pairs. As final payoff, the distributors received their income from the first phase, while the non-distributors received what had been distributed to them by the randomly chosen distributor.

[8]While this may generate a demand effect on how to distribute income from different sources, this does not generate a demand effect for the main question addressed in this study, namely, if there is any difference in distributive decisions between individuals with different relative income.

[9]See instructions on page 145 and page 146 for screenshots of the computer interface.

3.2.2 Procedural details

In total, 262 subjects participated in the experiment. We conducted 8 sessions with 32 to 34 participants each. The experiment lasted about an hour. It took place at the computer lab of the University of Zurich, Switzerland, in March and April 2014. We recruited our participants from local university students, excluding economics and psychology majors.[10] To program and conduct the experiment, we used the software z-Tree (Fischbacher, 2007). The instructions used neutral language, avoiding terms like "tournament", "winner", or "distributor".[11] We kept the participants' identity and their decisions anonymous throughout the experiment. The average payoff was CHF 25 (ca. US$ 28), including a participation fee of CHF 10 (ca. US$ 11). We paid all payoffs individually and in private immediately after the experiment.

3.2.3 Identification strategy

We use the distributors' decisions in the distribution phase to infer their views on what is a fair distribution of income – both for situations in which income differences are due to luck (the lottery treatment) and for situations in which income differences are due to effort (the tournament treatment).[12]

We believe that the distributive decisions identify the participants' fairness views for the following reasons: First, since the distributors' incomes were unaffected by their own distributive decision, there was no self-interest involved in the redistribution. Second, as the instructions made very clear that the distributors' incomes were not subject to any decisions by the non-distributors, the distributors had no reason to make accommodating distributive decisions in order to induce reciprocal behavior on the side of the non-distributors. Third, there was no reason for strategic decisions, since a distributor's decision would either be discarded or fully implemented. Fourth, distributive decisions were made only after the incomes had been generated. Furthermore, details about the distribution phase were unknown in the income generation phase. Therefore, distributive decisions had no incentive effects in the first stage.[13]

The variation of the distributors' income in the first phase allows us to identify the effect of relative income on their fairness views. First, we compare the distribu-

---

[10]The recruitment was conducted with the software hroot (Bock et al., 2012).

[11]The instructions are contained in Appendix A3.2.

[12]Clearly, the outcome of the tournament also depends on luck. Potential random factors are ability to handle the mouse, physical and mental state at the day of the experiment, and, most importantly, the performance of the opponent. This is, however, realistic in that there is no real-world income generation process which depends solely on effort. We therefore believe that it is still reasonable to interpret income from the tournament as income acquired through effort.

[13]We cannot rule out that subjects who are not motivated by any fairness consideration made their distributive choices at random. However, this only adds noise to the data and thus, if anything, makes it harder to find any effect.

tive decisions of those with high income from the lottery (HiLot) and those with low income from the lottery (LoLot). Since the selection of participants into both types was random, this gives us the causal effect of relative income from luck. Second, to obtain the effect of relative income from effort, we compare the decisions of those with high income from the tournament (HiTour) and those with low income from the tournament (LoTour). In this comparison, however, we need to control for self-selection. This is because more competitive or able individuals, who are more likely to end up with high income from the tournament by exerting systematically higher effort, may have different fairness views than others. As a consequence, differences in fairness views between HiTour and LoTour could simply result from a higher share of competitive or more able individuals among type HiTour. We control for self-selection by exploiting the random variation in income between individuals with similar effort in the tournament. Indeed, among individuals with similar effort, some were randomly matched with a participant with lower effort than theirs and thus received a high income, while others were randomly matched with a participant with higher effort than theirs and thus received a low income. The causal effect of income from effort can be identified through the randomness of this matching.[14] Further details about our strategy to control for selection will be given below.

Table 3.2: Identification strategy

Comparison	Effect
HiLot vs. LoLot	income from luck
HiTour vs. LoTour	income from effort
HiLot vs. HiTour	income generation for high income
LoLot vs. LoTour	income generation for low income

By keeping the distributors' income levels constant and varying the process that generated their income, we test whether there is an effect of the source of income on people's fairness views. This means that we separately compare the distributive decisions of those with high income (HiLot and HiTour) and of those with low income (LoLot and LoTour). While the selection into both income generation processes was random, there has still been a self-selection of individuals into both types from the tournament (HiTour and LoTour). Therefore, we also control for selection in the latter two comparisons. A design feature that facilitates this is the fact that all participants, before being allocated to the treatments, were required to complete the effort task. While this feature probably increased the discrepancy between the experimental and the natural environment, it did not only allow us to control for selection, but also avoided that individuals who were in the tournament treatment

[14] Note that if there were no randomness involved in the generation of income from effort, it would not be possible to identify any causal effect.

had an informational advantage about the experience of solving the task. The latter could have led to different redistributive choices compared to individuals in the lottery treatment.[15] Table 3.2 summarizes our identification strategy.

3.3 Results

First of all, we confirm the well-established result that inequalities which are due to effort tend to be accepted more than inequalities which are due to luck (Cappelen et al., 2007; Almås et al., 2010; Cappelen et al., 2010; Krawczyk, 2010; Rustichini & Vostroknutov, 2014; Kataria & Montinari, 2012; Vostroknutov et al., 2012; Cappelen et al., 2013; Durante et al., 2014).[16] As Figure 3.1 shows, the average amount distributed to the individual with low income from the tournament (LoTour) is approximately 40 percent lower (signed-rank p=0.00) than the average amount distributed to the participant with low income from the lottery (LoLot). Thus, we can be confident that the distributive situations induced in our experiment are comparable to those in earlier studies, and that even though the distributors did not have any material interest at stake, their fairness motives were strong enough to incentivize their redistributive choices.

We now focus on the differences in distributive decisions between types. Out of our 230 distributors, we observe 59 of type HiLot and 59 of type LoLot, as well as 56 of type HiTour and 56 of type LoTour. Figure 3.2 shows the distributive decision of all four types for the pair from the lottery (left panel) and for the pair from the tournament (right panel).

[15] It is important to mention, however, that we cannot fully rule out informational asymmetries between participants in different treatments. More specifically, in the tournament treatment participants could infer from their income whether the effort of the participant they were matched with was higher or lower than theirs. Therefore, compared to individuals in the lottery treatment, individuals in the tournament treatment had an informational advantage through their performance in the task relative to others. It is not clear, however, how this might affect the comparison we are interested in. On the other hand, the alternative – also informing the participants in the lottery treatment about how their performance in the task compared to the performance of the participant they were matched with – bears the risk of mitigating the treatment effect: Lottery winners may not feel as a winners anymore if they knew with certainty that they would have won the tournament anyway.

[16] Consistent with these experimental findings are the results of Fong (2001), Bullock et al. (2003), Alesina and La Ferrara (2005), Alesina and Fuchs-Schündeln (2007), Isaksson and Lindskog (2009) and Alesina and Giuliano (2011). Using survey data, they show that preferences for redistribution are increasing in the individuals' belief to which degree economic outcomes are the result of luck rather than of effort. They show that preferences for redistribution are decreasing in the individuals' level of subjective freedom.

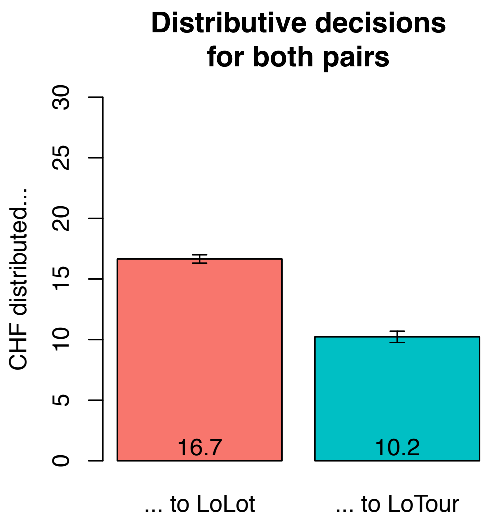


Figure 3.1: Decisions for both distributive situations. Heights of bars and values at bottom of bars correspond to means of amount (in CHF) distributed to the participant with low income. Lengths of whiskers at top of bars are equal to standard errors of the means.

3.3.1 Relative income from luck

We first analyze the effect of relative income from luck. On average, HiLot distributes about CHF 2.5 or 13 percent less to the non-distributor of type LoLot than LoLot does (rank-sum p=0.01). Surprisingly, LoLot distributes even significantly more than half of the total income that was earned in the pair to LoLot, which essentially reverses the inequality in this pair (CHF 18.5, signed-rank p=0.00).[17] This result, which could be interpreted as in-group bias or spite towards lottery winners on the side of the lottery losers, is consistent with previous evidence.[18] On the contrary, we find no difference in how much HiLot and LoLot distribute to the non-distributor of type LoTour (rank-sum p=0.63).[19] We conclude:

Result 3.1. *We find a causal effect of relative income from luck on fairness views. The effect is such that individuals with low income from (lack of) luck redistribute significantly more than individuals with high income from luck when inequalities are due to luck.*

[17] The amount distributed to LoLot by HiLot is not significantly higher than half of the total income (signed-rank p=0.16).

[18] Rustichini and Vostroknutov (2014) find in a different experimental setting that individuals are willing to reduce the lottery winnings of others at a cost to themselves.

[19] See Figure A3.1 for histograms of the distributive decisions of HiLot and LoLot for the pair from the lottery.

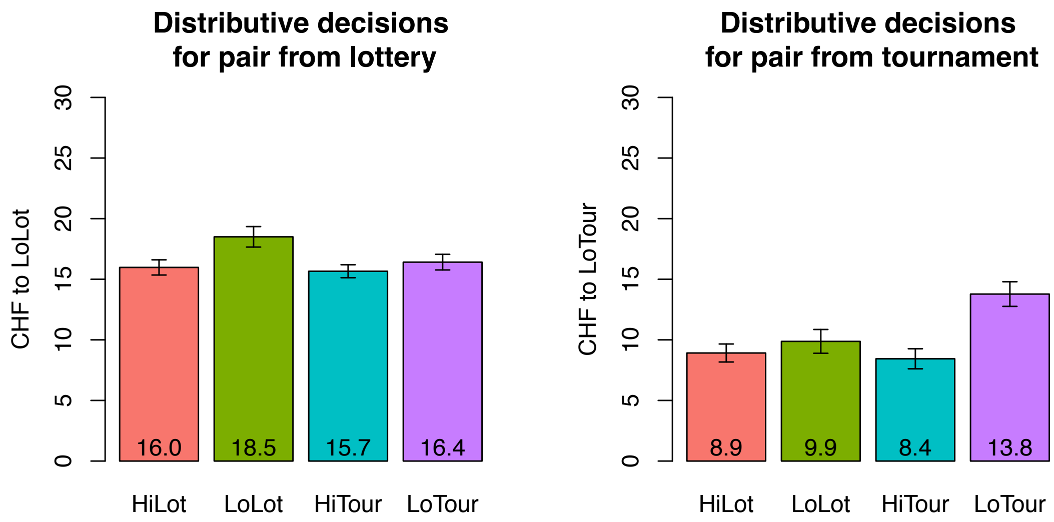


Figure 3.2: Distributive decisions of the four types. Heights of bars and values at bottom of bars correspond to means of amount (in CHF) distributed to the participant with low income. Lengths of whiskers at top of bars are equal to standard errors of the means.

3.3.2 Relative income from effort

Next, we consider the effect of relative income from effort by comparing the distributive decisions of HiTour and LoTour. There is no difference in how much both distribute to the non-distributor of type LoLot (rank-sum p=0.43). However, HiTour distributes about CHF 5 or 40% less to the non-distributor of type LoTour than LoTour does.[20] This difference is significant (rank-sum p=0.00), but, as argued above, may be due to selection effects.

In order to control for potential selection, we regress the amount in CHF distributed to the participant with low income from the tournament on a dummy variable for HiTour, and various controls for the distributors' effort in the income generation phase. We use linear and quadratic specifications, as well as fixed effects for effort bins of different sizes. An effort bin of size x means that we split the range of observed effort levels (0 to 169) into intervals of size x, and allow for a common fixed effect among all distributors whose effort levels are in the same interval. This keeps the distributors' effort level constant, so that the dummy variable captures only the variation that is caused by switching from low income to high income. This variation is purely exogenous: If the distributors' effort is fixed, whether they received high income in the tournament depends on the random event of whether they had been matched with a partner who had a lower effort than themselves. As a

[20]See Figure A3.2 for histograms of the distributive decisions of HiTour and LoTour for the pair from the tournament.

result, the coefficient of the dummy variable is an unbiased estimate of the effect of income from effort, while the coefficients of the various controls measure the selection effect. Note that an alternative interpretation of the coefficient of the dummy variable is that it measures the effect of luck that made effort pay off.

Table 3.3: Effect of income from effort on CHF distributed to LoTour

Control for effort	None	Linear	Quadratic	FE(10)	FE(5)	FE(2.5)
Constant	13.78***	16.16***	18.33***	15.00**	15.00**	15.00**
	(0.93)	(2.60)	(4.27)	(7.03)	(7.09)	(7.28)
HiTour	-5.34***	-4.50***	-4.72***	-3.92**	-3.69*	-4.05*
	(1.31)	(1.57)	(1.61)	(1.76)	(1.91)	(2.13)
F-test contr. (p)		0.330	0.506	0.677	0.725	0.844
N	112	112	112	112	112	112
R^2	0.13	0.14	0.14	0.22	0.29	0.38
Adj. R^2	0.12	0.12	0.12	0.10	0.08	0.03

Ordinary least squares regressions. Dependent variable: CHF distributed to LoTour. Sample: HiTour and LoTour. FE(x) means fixed effects for effort bins of size x. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors in parentheses.

Table 3.3 shows the regression results. They suggest that the difference in distributive decisions between HiTour and LoTour is not caused by selection, but by realized income itself. In fact, the estimate of the coefficient of the dummy variable is always negative and at least marginally significant. Furthermore, we can never reject the joint hypothesis that the coefficients of the controls are all zero (see row "F-test contr. (p)" in Table 3.3), suggesting that effort, and thus selection, has no predictive power. We believe that a bin size of 2.5 is sufficient to control for selection. Otherwise, one would have to argue that distributors whose effort levels differ by 2.5 or less units both exhibit a systematically different probability of receiving high income and differ significantly in their fairness views – a case that is unlikely.[21,22] Hence, we obtain:

Result 3.2. *We find a causal effect of relative income from effort on fairness views. The effect is such that individuals with high income from effort are less averse to inequalities that are due to effort than individuals with low income from (lack of)*

[21] With a bin size of 5 or 2.5, the coefficient of the dummy variable is only marginally significant. Note that there is a tradeoff when decreasing the bin size further: While the control of the selection effect improves, the variation in the dummy variable decreases, because there are fewer distributors with different income levels per bin. Hence, an explanation for the lower significance with a bin size smaller than 10 is the limited sample size.

[22] Note that the amount distributed to HiLot is restricted to the interval $[0, 30]$. When using a Tobit model rather than ordinary least squares to take this censoring into account, the estimate of the coefficient of the dummy variable is always significant.

effort. The effect is entirely driven by the luck that made the effort of high-income individuals pay off.[23]

3.3.3 Income generation

We now investigate whether, keeping the amount of income constant, the source of this income has an effect on fairness views. When comparing the decisions of distributors with high income (HiLot and HiTour), we find no difference in how much both distribute to LoLot and to LoTour (ranks-sum p=0.58 and p=0.45), suggesting that there are no differences in distributive decisions between income generation processes for high income. However, when we compare the decisions of distributors with low income (LoLot and LoTour), the picture is different. On the one hand, LoLot distributes more to non-distributors of type LoLot than LoTour does. This difference is close to being marginally significant (rank-sum p=0.10).[24] On the other hand, LoTour distributes significantly more to non-distributors of type LoTour than LoLot does (rank-sum p=0.01). To control for selection, we proceed analogously as for the effect of income from effort and regress the amounts distributed to each participant on a dummy for LoTour, as well as on various controls for effort. Table A3.1 and Table A3.2 show the results. The estimates for the coefficients of the dummy variable are at least marginally significant with linear and quadratic controls for effort.[25] We conclude:

Result 3.3. *There is a causal effect of the income generation process on the fairness views of low-income individuals, as the latter become more averse against the sources of inequalities that made themselves poor. There is no evidence for an effect of the income generation process on the fairness views of high-income individuals.*

3.4 Channel

Different explanations, not necessarily mutually exclusive, can account for the observed variation in the redistributive decisions, and thus in the fairness views, of the four types of distributors. One explanation for the results is a self-serving bias in attribution of responsibility. Earlier research has demonstrated that people take credit for personal successes and deny responsibility for failures (Miller & Ross,

[23]This contrasts with Erkal et al. (2011), who attribute their finding of a negative relationship between giving and income primarily to self-selection.
[24]When using a t-test, we obtain marginal significance of the difference (p=0.05).
[25]When using the Tobit model, we also obtain a marginally significant estimate when using fixed effects for effort bins of size 10.

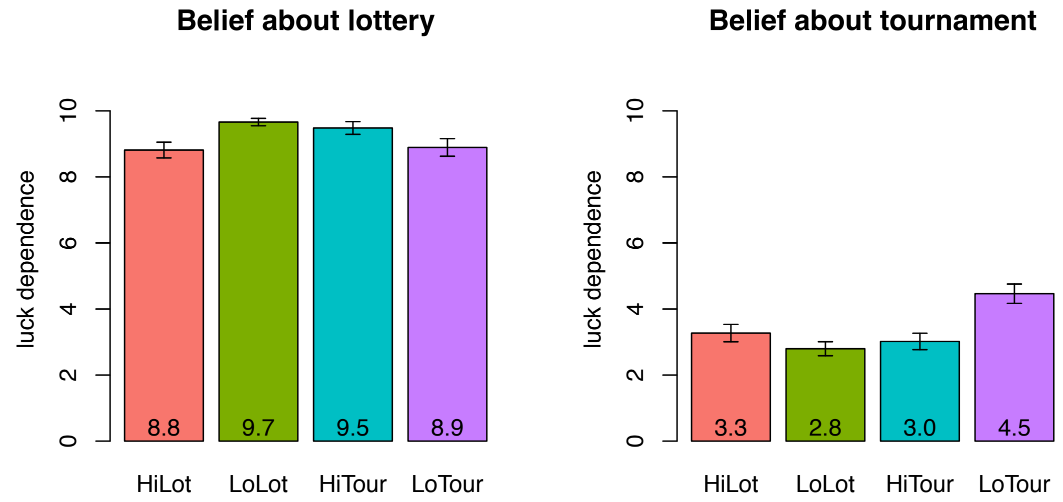


Figure 3.3: Beliefs about income generation processes. Heights of bars and values at bottom of bars correspond to means of belief about luck dependence of the process. Lengths of whiskers at top of bars are equal to standard errors of the means.

1975; Bradley, 1978). More specifically, successful individuals tend to attribute their outcome to circumstances under their control (so-called internal factors), whereas unsuccessful individuals tend to attribute their outcome to circumstances not under their control (so-called external factors). For our experiment, this would predict that high-income individuals from the tournament (HiTour) tend to attribute the outcome of the tournament more to internal factors than low-income individuals from the tournament (LoTour). Analogous predictions would hold for high-income and low-income individuals from the lottery (HiLot and LoLot).[26] Given the well-established result that individuals favor a more equal distribution of income when the latter is due to luck – an external factor – rather than effort or choice – which are internal factors – (see discussion in Section 3.3), this would explain our observations for redistributive behavior: If distributors of type HiTour attribute the outcome of the tournament less to external factors than LoTour, they would redistribute less than LoTour in the pair of non-distributors whose incomes were generated in the tournament. A similar argument would hold for the distributors of type HiLot and LoLot.

We test this hypothesis of self-serving bias by analyzing the participants' responses to a survey conducted at the end of the experiment. Participants were

[26]It might seem absurd at first why anyone would believe that internal factors play a role in the lottery – whose outcome is random. However, certain philosophies such as Buddhism view an individual's fortune as the result of past actions. Hence, it is at least possible that some high-income individuals from the lottery attribute their success to such internal factors. This perspective would deny pure randomness of the lottery.

asked to state, on a scale from 1 to 10, to what extent they thought that the outcome in the tournament treatment was due to effort rather than to luck, where "1" represented "all due to effort" and "10" represented "all due to luck". Similarly, the participants were asked to choose, from 1 to 10, to what extent they thought that the outcome in the lottery treatment was due to "individual attributes (e.g. karma, religiousness)" rather than to luck, where "1" represented "all due to individuals attributes" and "10" represented "all due to luck".[27]

Figure 3.3 shows the average beliefs of all four types of distributors regarding the lottery (left panel) and the tournament (right panel).

Consistent with the self-serving bias hypothesis, we find that HiTour distributors believe significantly less (rank-sum p=0.00) in the luck dependence of the tournament than LoTour distributors do. This result does not change when controlling for self-selection (see Table A3.3).[28] Similarly, although to a smaller extent, HiLot distributors believe significantly less (rank-sum p=0.00) in the luck dependence of the lottery than LoLot distributors do.[29] While the latter might seem surprising, as individuals cannot be responsible for the outcome of a lottery, this is consistent with previous evidence by Gurdal et al. (2013), which shows that principals blame agents for the outcome of a lottery regardless of the agents' choices. We obtain:[30]

Result 3.4. *High-income individuals believe less in the luck dependence of their outcomes compared to low-income individuals.*

Thus, Result 3.4 identifies a self-serving bias on responsibility attribution as a potential channel of the effect of relative income and of its generation process on individuals' fairness views.

Note that another explanation for the variation in the redistributive decisions is a direct in-group bias: Be nicer to those who are similar to yourself. If individuals with the same relative income and the same process of income generation develop a group-identity feeling, this may explain why, on average, non-distributors receive more from a distributor of their own type than from a distributor of a competing type.[31] This applies in particular to low-income individuals from lack of luck, who redistribute significantly more than half to low-income individuals from the lottery.

[27] We always asked the question about the outcome of the tournament treatment first.

[28] HiTour and LoTour also differ in their belief regarding the lottery (rank-sum p=0.02).

[29] For histograms of the beliefs, see Figure A3.3 and Figure A3.4.

[30] Similar results follow when comparing beliefs between individuals with the same income, but different income generation process. LoTour believes significantly more in the luck dependence of the tournament than LoLot (rank-sum p=0.00), and vice-versa (rank-sum p=0.01). HiLot attributes outcomes from the lottery significantly more to internal factors than HiTour (rank-sum p=0.00). The reverse, however, is not true (rank-sum p=0.56).

[31] On average, HiLot receives CHF 14.0 from HiLot, but only CHF 11.5 from LoLot. Similarly, LoLot receives CHF 18.5 from LoLot, but only CHF 16.0 from HiLot. Analogous comparisons hold for HiTour and LoTour: HiTour receives CHF 21.6 from HiTour, but only CHF 16.2 from LoTour. LoTour receives 13.8 from LoTour, but only 8.4 from LoLot.

Previous evidence on redistributive choices has indeed shown the effect of in-group favoritism based on race (Luttmer, 2001), on age and gender (Cardenas & Sethi, 2010), on risk-taking choices (Costard & Bolle, 2011), and on field of studies (Klor & Shayo, 2010). In our experiment, distributors had only information about the individuals' relative income and the process that generated that income. Therefore, these were the only characteristics that may have generated a group-identity feeling. Nevertheless, we cannot rule out that a pure in-group bias is also playing a role in the redistributive decisions (especially for low-income individuals from the lottery) besides the self-serving bias on responsibility attribution, as both channels cannot be distinguished in our data.[32]

Finally, we would like to point out a further implication of Result 3.4: It suggests that the individuals' experience in terms of their economic outcomes affects their beliefs about how these outcomes have come about. This aligns well with previous papers that emphasize an effect of past experience, such as communism (Alesina & Fuchs-Schündeln, 2007) and recessions (Giuliano & Spilimbergo, 2014), on beliefs.[33] We add to this literature by showing that idiosyncratic events, not only phenomena affecting the society as a whole, play a role in the formation of beliefs about inequality, too. Furthermore, our findings suggest a substantial degree of subjectivity in these beliefs: Even when the luck dependence of an income generation process in known and fixed (as in the lottery), there are individuals whose beliefs deviate from this objective truth (HiLot).

3.5 Conclusion

In this experiment, we vary the participants' income and the way their income is generated. We then elicit their fairness views through their redistributive decisions over other participants' incomes. We find a causal effect of relative income on fairness views. This effect is such that in comparison to high-income individuals, low-income individuals redistribute significantly more when the source of inequalities is the same as the one that made themselves poor. We then argue that an explanation for this result is a self-serving bias on responsibility attribution, which is supported by our data: Compared to low-income individuals, high-income individuals tend to believe more that their outcome is the result of internal rather than external factors.

Our results suggest that personal income changes individuals' views about a fair distribution of income in society. More specifically, we show that personal income

[32] When regressing the amount distributed to LoTour on a dummy variable for HiTour and beliefs for the tournament, both variables have explanatory power for the distributive decisions. This suggests that income affects distributive choices not only through a change of beliefs. Therefore, self-serving biases in the beliefs may not the be the only relevant channel.

[33] Theoretical papers focusing on belief formation and how it affects redistributive policies are Piketty (1995), Alesina and Angeletos (2005) and Bénabou and Tirole (2006).

may increase the acceptance of income differences. This would create conflicting ideologies between rich and poor and increase the discrepancy in preferences for redistribution beyond what is already caused by selfish motives. As a consequence, rising income inequality, by increasing the polarization in political preferences, is likely to make it even harder for societies to reach a consensus about redistribution in the long run.

One should, however, always be cautious when generalizing from the results of a particular study. Note that what we identify is essentially an effect of relative income made in an *experiment* on individuals' views about what is a fair distribution of income from that *experiment*. Of course, the fair distribution of income in society is a much more fundamental question than that of a fair distribution of experimental income. Furthermore, the experiment was designed to test the validity of some hypotheses in isolation, namely, in the absence of additional features that may play a role in a natural environment. For instance, in the field, individuals do not actually make direct redistributive decisions over other people's incomes, but use their voting right to influence redistribution. Furthermore, their voting decisions often affect their personal income in addition to the income of others. While new treatments can always be run to study the role of each of these environmental features, it is precisely the absence of these features in our experiment that allowed us to rule out selfish motives and strategic behavior and, thus, to identify the causal effect of relative income on individuals' fairness views. These fairness views have been shown to be a significant determinant of preferences for redistribution in the laboratory and in the field. Therefore, we believe that our study, by increasing our understanding of how these fairness views may be formed, leaves a message that is relevant for future theoretical and empirical studies on distributive justice: Fairness views about income distribution should not be treated as exogenous, as they are likely to depend on individuals' relative income.

Acknowledgements

We are grateful to Dan Acland, Ned Augenblick, Björn Bartling, Jean-Michel Benkert, Alain Cohn, Gregory Crawford, Ernesto Dal Bó, Florian Engl, Nadja Jehli, Johannes Maier, John Morgan, Thomas Palfrey, Aldo Rustichini, Armin Schmutzler, Bertil Tungodden, Roberto Weber, Noam Yuchtman, and seminar participants in Berkeley (Goldman School of Public Policy, Haas School of Business), Denver (Annual Conference of the Western Economic Association), Geneva and Lausanne (Swiss Young Researchers Workshops on Behavioral Economics and Experimental Research), Stanford (Bay Area Behavioral and Experimental Economics Workshop), Solothurn (Zurich Workshop on Economics), and Zurich for helpful discussions and suggestions. Klein gratefully acknowledges financial support of the Swiss National Science Foundation (grant numbers 131854 and 151688).

Appendix

A3.1 Tables and figures

Table A3.1: Effect of income generation on CHF distributed to LoLot

Control for effort	None	Linear	Quadratic	FE (10)	FE(5)	FE(2.5)
Constant	18.50***	20.86***	23.22***	16.93***	17.51***	17.21***
	(0.75)	(2.05)	(3.03)	(5.74)	(5.83)	(5.76)
LoTour	-2.09*	-2.44**	-2.19*	-1.93	-2.51*	-2.21*
	(1.07)	(1.11)	(1.13)	(1.17)	(1.27)	(1.30)
F-test contr. (p)		0.218	0.270	0.206	0.397	0.318
N	115	115	115	115	115	115
R^2	0.03	0.05	0.06	0.19	0.28	0.40
Adj. R^2	0.02	0.03	0.03	0.06	0.04	0.07

Ordinary least squares regressions. Dependent variable: CHF distributed to LoLot. Sample: LoLot and LoTour. FE(x) means fixed effects for effort bins of size x. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors in parentheses.

Table A3.2: Effect of income generation on CHF distributed to LoTour

Control for effort	None	Linear	Quadratic	FE(10)	FE(5)	FE(2.5)
Constant	9.87***	15.50***	16.13***	12.31	13.31*	12.86
	(0.99)	(2.66)	(3.96)	(7.62)	(7.73)	(7.78)
LoTour	3.90***	3.06**	3.13**	2.69*	1.69	2.14
	(1.41)	(1.44)	(1.48)	(1.55)	(1.68)	(1.76)
F-test contr. (p)		0.025	0.080	0.258	0.438	0.496
N	115	115	115	115	115	115
R^2	0.06	0.10	0.11	0.21	0.29	0.39
Adj. R^2	0.05	0.09	0.08	0.08	0.06	0.05

Ordinary least squares regressions. Dependent variable: CHF distributed to LoTour. Sample: LoLot and LoTour. FE(x) means fixed effects for effort bins of size x. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors in parentheses.

Table A3.3: Effect of income from effort on belief about tournament

Control for effort	None	Linear	Quadratic	FE(10)	FE(5)	FE(2.5)
Constant	4.46***	4.48***	5.68***	7.00***	7.00***	7.00***
	(0.27)	(0.77)	(1.25)	(1.96)	(1.94)	(1.97)
HiTour	-1.45***	-1.44***	-1.56***	-1.41***	-1.48***	-1.38**
	(0.38)	(0.46)	(0.47)	(0.49)	(0.52)	(0.58)
F-test contr. (p)		0.980	0.484	0.092	0.102	0.279
N	112	112	112	112	112	112
R^2	0.11	0.11	0.13	0.28	0.37	0.46
Adj. R^2	0.11	0.10	0.10	0.17	0.19	0.16

Ordinary least squares regressions. Dependent variable: Belief about luck dependence of tournament. Sample: HiTour and LoTour. FE(x) means fixed effects for effort bins of size x. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors in parentheses.

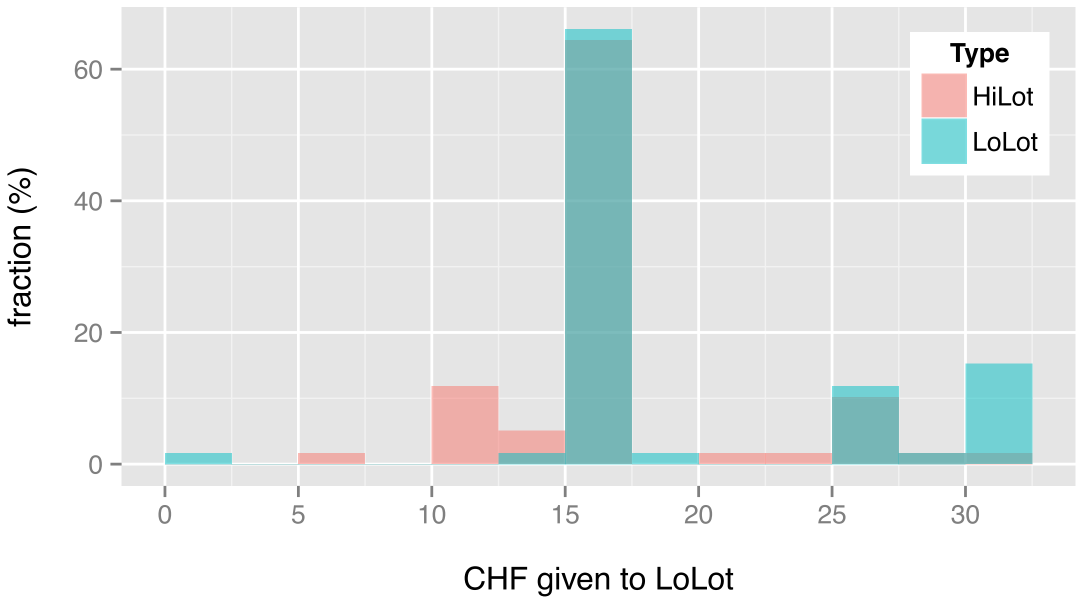


Figure A3.1: Histograms of distributive decisions of HiLot and LoLot for pair from lottery

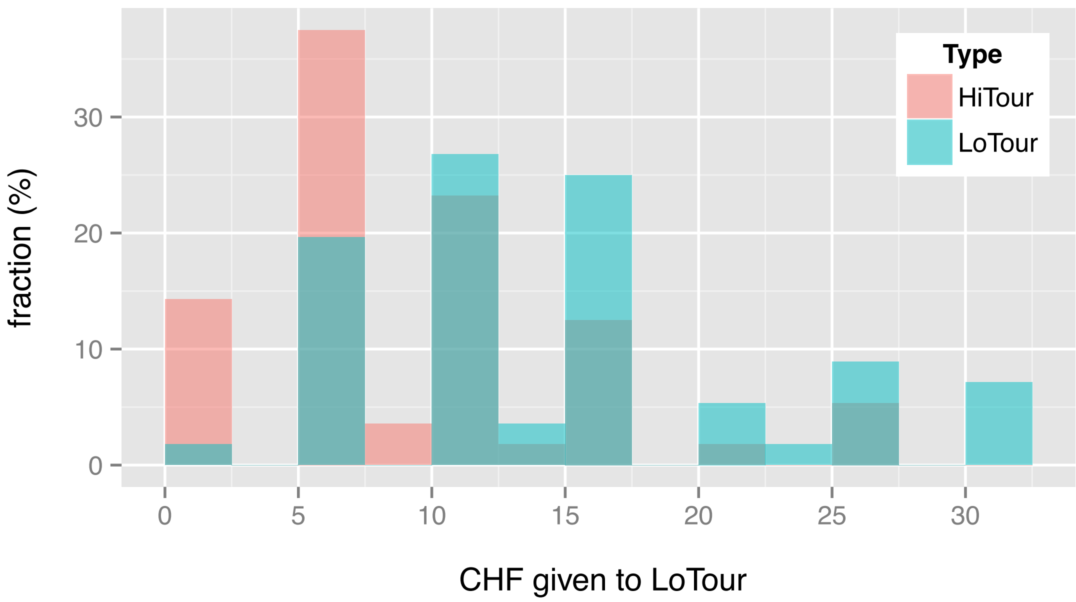


Figure A3.2: Histograms of distributive decisions of HiTour and LoTour for pair from tournament

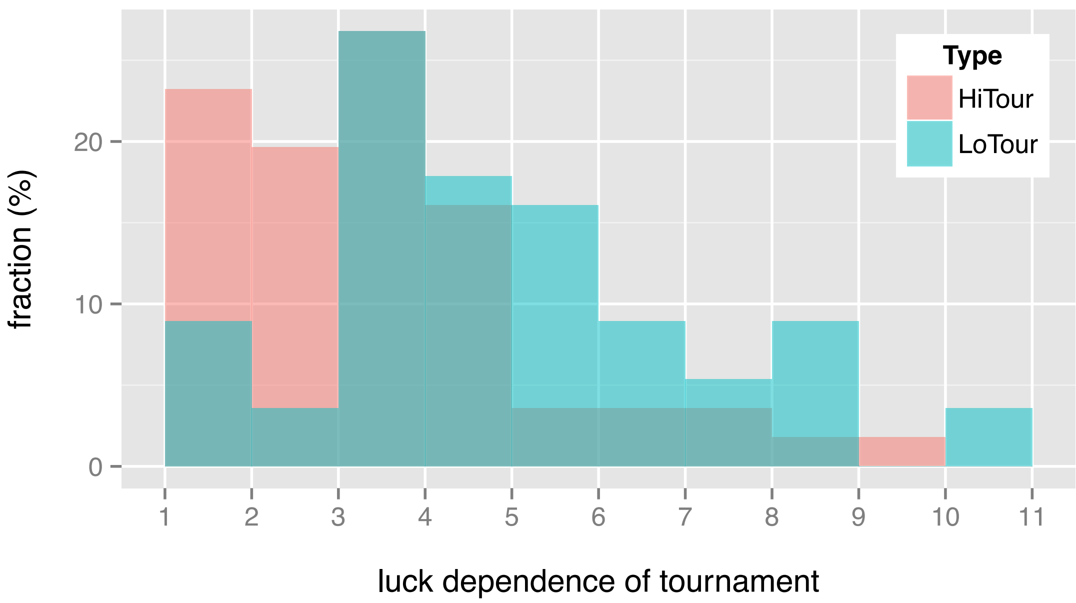


Figure A3.3: Histograms of beliefs of HiTour and LoTour for tournament

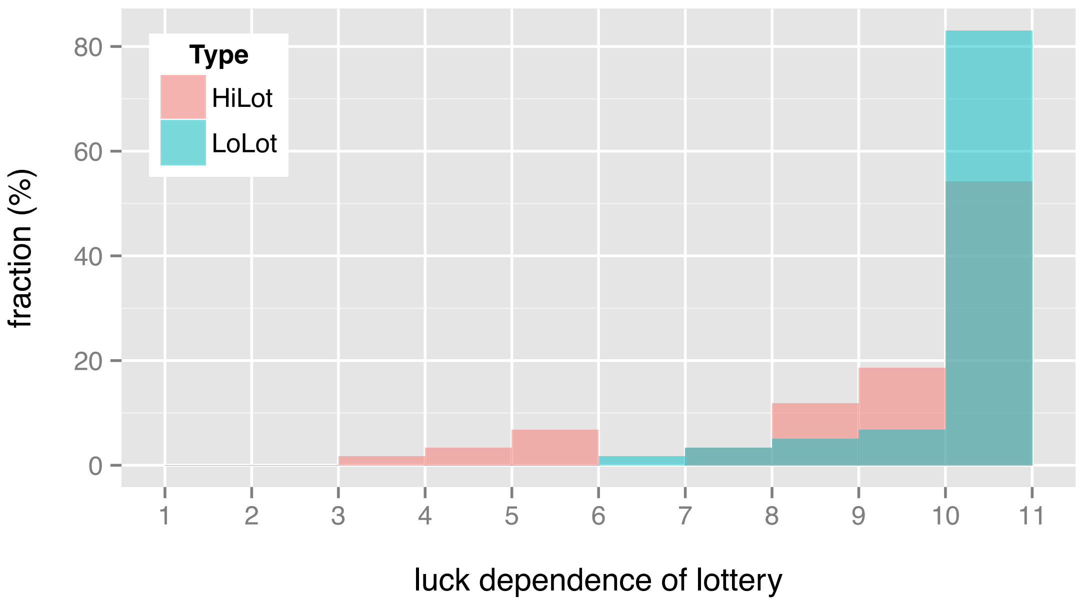


Figure A3.4: Histograms of beliefs of HiLot and LoLot for lottery

A3.2 Instructions

General Instructions

Introduction

Welcome and thank you for participating in this experiment. During the next 60 minutes, you will make decisions that determine your earnings and the earnings of other participants. You will also receive a fixed **participation fee** of CHF 10. Upon completion of the experiment, you will be paid all of your earnings and the participation fee, individually and in private.

Anonymity

All of your interactions with other participants are completely anonymous. You will never learn the identity of the participants with whom you interact. They will also never learn your identity. You will not know which choices were made by a specific participant and no other participant will know which choices were made by you.

Rules of Conduct

During the experiment, you are not allowed to communicate with other participants, exclaim, use personal electronic devices, or use the computer in a way not specified by the experimenter. If you are not following these rules, you may be excluded from the experiment

Phases

In this experiment, there are two phases. We will now describe Phase 1. Details about Phase 2 will be provided after Phase 1 is completed.

1

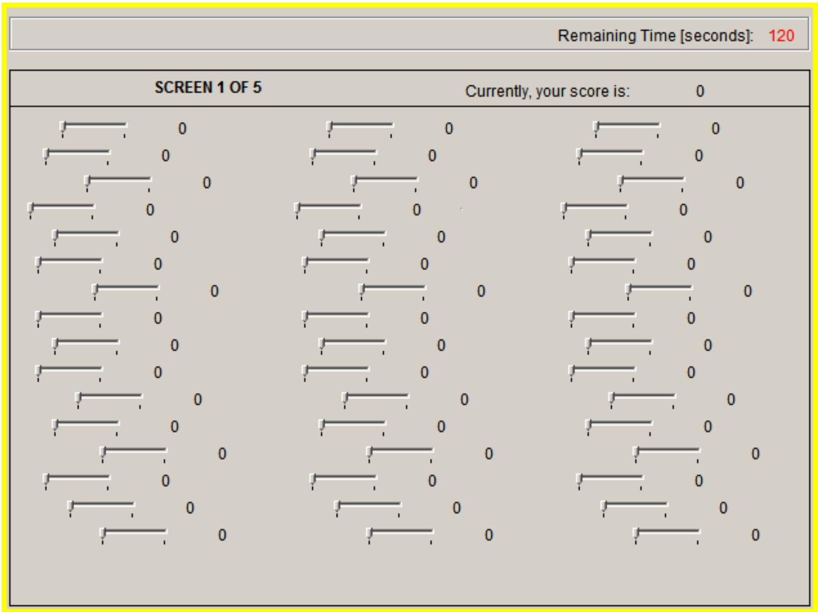
Instructions for Phase 1

<u>Pairs</u>

At the beginning of Phase 1, you will be randomly paired with another participant. In the following, we will refer to the participant you are paired with as "*your paired participant*."

<u>Task</u>

At the beginning of Phase 1, all participants will individually complete a **task**. The task will consist of a sequence of 5 screens with 48 sliders each (see picture of the computer screen below).



Each slider is initially positioned at 0 and can be moved as far as 100. Each slider has a number to its right showing its current position. You can use the mouse in any way you like to move each slider. You can readjust the position of each slider as many times as you wish. **Your goal is to position as many sliders as possible at exactly 50**. For each screen, you have **120 seconds** to position all the sliders. After the 120 seconds are over, a new screen will appear. In total, 5 screens will be presented to you. The total number of sliders positioned at exactly 50 in the 5 screens represents your **score** in the task.

Earnings generation

After all participants have completed the task, the computer will randomly assign your pair, namely you and your paired participant, to one out of two earning rules. Each pair is equally likely to be assigned to each of the two earning rules. The earning rule to which your pair is assigned will then determine your earnings and the earnings of your paired participant. The two rules are the following:

1) SCORE RULE

If your pair is randomly assigned to the Score Rule, the participant in your pair who achieved a higher score in the task receives earnings of CHF 25, and the participant who achieved a lower score in the task receives earnings of CHF 5. Thus, two cases are possible, depending on your score and the score of the other:

- Case 1: If your score in the task is **higher** than the score of your paired participant:

Your earnings are **CHF 25** The earnings of your paired participant are **CHF 5**

- Case 2: If your score in the task is **lower** than the score of your paired participant:

Your earnings are **CHF 5** The earnings of your paired participant are **CHF 25**

- In the case that your score and the score of your paired participant are equal, the computer will randomly determine who receives earnings of CHF 25 and who receives earnings of CHF 5.

2) LOTTERY RULE

If your pair is randomly assigned to the Lottery Rule, the computer will randomly determine who receives earnings of CHF 25 and who receives earnings of CHF 5. Thus, two cases are possible. Both cases are equally likely.

- Case 1 (probability 50%)

Your earnings are **CHF 25** The earnings of your paired participant are **CHF 5**

- Case 2 (probability 50%)

Your earnings are **CHF 5** The earnings of your paired participant are **CHF 25**

Therefore, if your pair is randomly assigned to the Lottery Rule, **your score in the task has no effect on your earnings or on the earnings of your paired participant in Phase 1.**

Remember that which of the two rules will generate the earnings in your pair will be randomly determined after the task has been completed. More specifically, the computer will randomly assign half of the pairs in the experiment to the Score Rule and half of the pairs in the experiment to the Lottery Rule. In case of an uneven number of pairs, the remaining pair will be randomly assigned either to the Score Rule or to the Lottery Rule.

Practice round

Before you complete the actual task, all participants will be asked to practice the task for a period of 60 seconds. The screen will look exactly as in the actual task. The purpose of the practice round is to make participants familiar with the task. Thus, the score that you achieve during the practice round has no effect on your earnings or on the earnings of your paired participant in Phase 1.

Overview of Phase 2

Phase 2 of the experiment concerns the distribution of earnings from Phase 1. Details of Phase 2 will be provided after Phase 1 is complete.

This concludes the General Instructions and the Instructions for Phase 1.

If there are any questions now or at any point during the experiment, please raise your hand, and one of us will approach you individually.

Summary of Phase 1

- All participants complete the task.
- Computer randomly assigns pairs either to the Score Rule or to the Lottery Rule.
- Earnings in Phase 1 are determined according to the assigned rule.

Instructions for Phase 2

Types

At the beginning of Phase 2, the computer will randomly assign your pair, consisting of you and your paired participant, to one of two types: **Type D (Distributors)** or **Type N (Non-Distributors)**.

More specifically, all pairs will be of Type D, with the exception of two randomly selected pairs who will be of Type N: one pair whose earnings in Phase 1 were generated through the Score Rule and one pair whose earnings in Phase 1 were generated through the Lottery Rule.

To summarize, there will be two pairs who are of Type N: one pair from the Score Rule and one pair from the Lottery Rule. These two pairs will be randomly selected.

Participants in the other pairs will be of Type D.

Decisions and Earnings of Type N participants

Type N participants will NOT make any decisions in this phase. However, their earnings may be affected by decisions made by Type D participants in Phase 2.

Decisions and Earnings of Type D participants

All **Type D** participants will be asked to make decisions about the distribution of earnings of Type N participants from Phase 1 (a detailed explanation of how these choices will be made follows below). The earnings of Type D participants will remain fixed, as they were at the end of Phase 1. This means that the earnings of Type D participants are not affected by anything that happens in Phase 2.

At the end of the experiment, the computer will randomly select one Type D participant whose decisions will then be applied to determine the final earnings of Type N participants. Therefore, Type D can affect the earnings of Type N participants, but nobody can affect the earnings of Type D participants.

Type D participants will make the following decisions about the distribution of earnings of Type N participants: **for each of the two randomly selected Type N pairs, Type D participants will be asked to divide the sum of earnings within the pair, namely CHF 30 (25+5), between the two participants forming the pair.**

So overall, Type D participants will make two decisions about the distribution of earnings from Phase 1:

- Each Type D participant will make one decision for the pair of Type N participants whose earnings in Phase 1 were generated through the *Score Rule*.

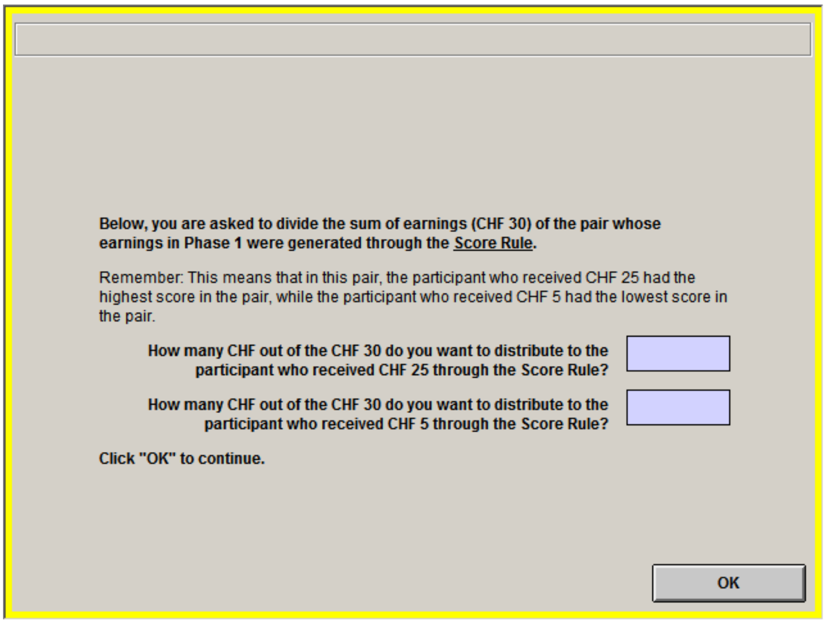
5

- Each Type D participant will make one decision for the pair of Type N participants whose earnings in Phase 1 were generated through the *Lottery Rule*.

Recall that each Type N pair ended Phase 1 with one participant whose earnings were CHF 25 and one participant whose earnings were CHF 5, so the sum of earnings within each pair is CHF 30. So Type D participants can distribute any amount between CHF 0 and CHF 30 to the two Type N participants in the pair, but the amounts distributed to both Type N participants must sum up to the sum of earnings of the pair, which is CHF 30. Note that all amounts must be multiples of CHF 0.5.

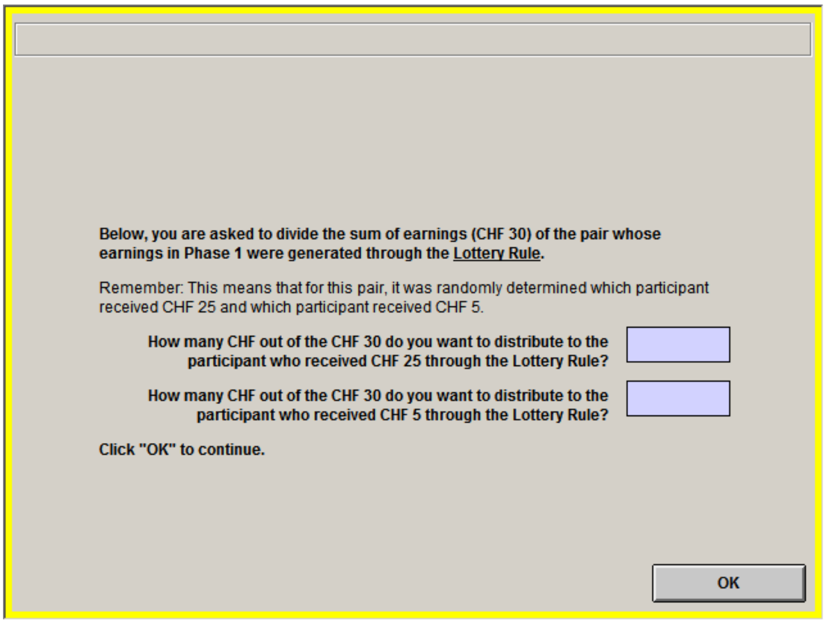
So for the pair of Type N participants whose earnings in Phase 1 were generated through the Score Rule, Type D participants will be asked to decide (see picture of the screen below)

- how many CHF out of the CHF 30 to distribute to the participant who received earnings of CHF 25 through the Score Rule, and
- how many CHF out of the CHF 30 to distribute to the participant who received earnings of CHF 5 through the Score Rule.



Similarly, for the pair of Type N participants whose earnings in Phase 1 were generated through the Lottery Rule, Type D participants will be asked to decide (see picture of the screen below)

- how many CHF out of the CHF 30 to distribute to the participant who received earnings of CHF 25 through the Lottery Rule, and
- how many CHF out of the CHF 30 to distribute to the participant who received earnings of CHF 5 through the Lottery Rule.



Remember that after all Type D participants have made their decisions, one Type D participant will be randomly selected, and **only** the decisions of this randomly selected Type D participant will be applied to determine the earnings of the Type N participants. This means that, if you are a Type D participant, your decisions may end up entirely determining which earnings each of the four Type N participants receives. At the end of the experiment, Type D participants will be informed whether their decisions were selected to count or not.

<u>Final payments to Type D participants after the experiment</u>

After the experiment, Type D participants will be paid their earnings from Phase 1, plus the participation fee of CHF 10. Phase 2 has **no** effect on their payments.

Final payments to Type N participants after the experiment

After the experiment, the participants in the two Type N pairs that were randomly selected by the computer will be paid the earnings that were distributed to them by one randomly selected Type D participant in Phase 2, plus the participation fee of CHF 10.

End of the experiment

Once everyone has completed Phase 2, the amount that will be paid to you will appear on your computer screen. Then, you will be asked to answer a few questions. When you are finished answering the questions, please wait patiently at your seat until you are called to collect your payment in private.

Summary of Phase 2

- The computer randomly selects 2 pairs, one from the Score Rule and one from the Lottery Rule, to be of Type N. The remaining pairs are of Type D.
- All Type D participants make decisions about the distribution of earnings of the two pairs of Type N participants from Phase 1.
- The computer randomly selects one Type D participant whose distributive decisions will count for the payment of the four Type N participants after the experiment.
- Experiment ends.
- Type D participants are paid their earnings from Phase 1.
- Type N participants are paid the amount distributed to them by the randomly selected Type D participant

References

Agranov, M. & Palfrey, T. R. (2014). Equilibrium tax rates and income redistribution: A laboratory study. NBER Working Paper No. 19918.

Alesina, A. & Angeletos, G.-M. (2005). Fairness and redistribution. *The American Economic Review*, 95(4), 960–980.

Alesina, A. & Fuchs-Schündeln, N. (2007). Good-bye Lenin (or not?): The effect of communism on people's preferences. *The American Economic Review*, 97(4), 1507–1528.

Alesina, A. & Giuliano, P. (2011). Preferences for redistribution. In J. Benhabib, M. O. Jackson, & A. Bisin (Eds.), *Handbook of Social Economics*, Vol. 1A (pp. 93–131). Amsterdam: North-Holland.

Alesina, A. & La Ferrara, E. (2005). Preferences for redistribution in the land of opportunities. *Journal of Public Economics*, 89(5-6), 897–931.

Almås, I., Cappelen, A. W., Sørensen, E. Ø., & Tungodden, B. (2010). Fairness and the development of inequality acceptance. *Science*, 328(5982), 1176–1178.

Barr, A., Burns, J., Miller, L., & Shaw, I. (2012). Economic status and acknowledgment of earned entitlement. Working paper. Retrieved 2015/01/15, from http://blogg.nhh.no/thechoicelab/wp-content/uploads/2012/10/Barr+Burns+Miller+Shaw+10+Sept+2012+with+figures.pdf.

Becchetti, L., Degli Antoni, G., Ottone, S., & Solferino, N. (2011). Spectators versus stakeholders with or without veil of ignorance: The difference it makes for justice and chosen distribution criteria. ECINEQ Working Paper No. 2011-204.

Bénabou, R. & Tirole, J. (2006). Belief in a just world and redistributive politics. *The Quarterly Journal of Economics*, 121(2), 699–746.

Bock, O., Nicklisch, A., & Baetge, I. (2012). hroot: Hamburg registration and organization online tool. WiSo-HH Working Paper No. 1.

Bradley, G. W. (1978). Self-serving biases in the attribution process: A reexamination of the fact or fiction question. *Journal of Personality and Social Psychology*, 36(1), 56–71.

Buckley, E. & Croson, R. (2006). Income and wealth heterogeneity in the voluntary provision of linear public goods. *Journal of Public Economics*, 90(4-5), 935–955.

Bullock, H. E., Williams, W. R., & Limbert, W. M. (2003). Predicting support for welfare policies: The impact of attributions and beliefs about inequality. *Journal of Poverty*, 7(3), 35–56.

Cappelen, A. W., Hole, A. D., Sørensen, E. Ø., & Tungodden, B. (2007). The pluralism of fairness ideals: An experimental approach. *The American Economic Review*, 97(3), 818–827.

Cappelen, A. W., Konow, J., Sørensen, E. Ø., & Tungodden, B. (2013). Just luck: An experimental study of risk-taking and fairness. *The American Economic Review*, 103(4), 1398–1413.

Cappelen, A. W., Sørensen, E. Ø., & Tungodden, B. (2010). Responsibility for what? Fairness and individual responsibility. *European Economic Review*, 54(3), 429–441.

Cardenas, J. C. & Sethi, R. (2010). Resource allocation in public agencies: Experimental evidence. *Journal of Public Economic Theory*, 12(4), 815–836.

Costard, J. & Bolle, F. (2011). Solidarity, responsibility and group identity. European University Viadrina Frankfurt (Oder), Department of Business Administration and Economics Discussion Paper No. 309.

Durante, R., Putterman, L., & van der Weele, J. (2014). Preferences for redistribution and perception of fairness: An experimental study. *Journal of the European Economic Association*, 12(4), 1059–1086.

Erkal, N., Gangadharan, L., & Nikiforakis, N. (2011). Relative earnings and giving in a real-effort experiment. *The American Economic Review*, 101(7), 3330–3348.

Fischbacher, U. (2007). z-tree: Zurich toolbox for ready-made economic experiments. *Experimental Economics*, 10(2), 171–178.

Fong, C. (2001). Social preferences, self-interest, and the demand for redistribution. *Journal of Public Economics*, 82(2), 225–246.

Frohlich, N., Oppenheimer, J., & Kurki, A. (2004). Modeling other-regarding preferences and an experimental test. *Public Choice*, 119(1-2), 91–117.

Gill, D. & Prowse, V. (2012). A structural analysis of disappointment aversion in a real effort competition. *The American Economic Review*, 102(1), 469–503.

Giuliano, P. & Spilimbergo, A. (2014). Growing up in a recession. *Review of Economic Studies*, 81(2), 787–817.

Gurdal, M. Y., Miller, J. B., & Rustichini, A. (2013). Why blame? *Journal of Political Economy*, 121(6), 1205–1247.

Isaksson, A.-S. & Lindskog, A. (2009). Preferences for redistribution – A country comparison of fairness judgements. *Journal of Economic Behavior & Organization*, 72(3), 884–902.

James, R. N. & Sharpe, D. L. (2007). The nature and causes of the U-shaped charitable giving profile. *Nonprofit and Voluntary Sector Quarterly*, 36(2), 218–238.

Kataria, M. & Montinari, N. (2012). Risk, entitlements and fairness bias: Explaining preferences for redistribution in multi-person setting. Jena Economic Research Paper No. 2012,061.

Klor, E. F. & Shayo, M. (2010). Social identity and preferences over redistribution. *Journal of Public Economics*, 94(3-4), 269–278.

Krawczyk, M. (2010). A glimpse through the veil of ignorance: Equality of opportunity and support for redistribution. *Journal of Public Economics*, 94(1-2), 131–141.

Luttmer, E. F. P. (2001). Group loyalty and the taste for redistribution. *Journal of Political Economy*, 109(3), 500–528.

Luttmer, E. F. P. & Singhal, M. (2011). Culture, context, and the taste for redistribution. *American Economic Journal: Economic Policy*, 3(1), 157–179.

Miller, D. T. & Ross, M. (1975). Self-serving biases in the attribution of causality: Fact or fiction? *Psychological Bulletin*, 82(2), 213–225.

Möllerström, J., Reme, B.-A., & Sørensen, E. Ø. (2014). Luck, choice and responsibility: An experimental study of fairness views. Research Institute of Industrial Economics Working Paper No. 1014.

Owens, L. A. & Pedulla, D. S. (2014). Material welfare and changing political preferences: The case of support for redistributive social policies. *Social Forces*, 92(3), 1087–1113.

Piff, P. K., Kraus, M. W., Côté, S., Cheng, B. H., & Keltner, D. (2010). Having less, giving more: The influence of social class on prosocial behavior. *Journal of Personality and Social Psychology*, 99(5), 771–784.

Piketty, T. (1995). Social mobility and redistributive politics. *The Quarterly Journal of Economics*, 110(3), 551–584.

Powdthavee, N. & Oswald, A. J. (2014). Does money make people right-wing and inegalitarian? A longitudinal study of lottery winners. IZA Discussion Paper No. 7934.

Rodriguez-Lara, I. & Moreno-Garrido, L. (2012). Self-interest and fairness: Self-serving choices of justice principles. *Experimental Economics*, 15(1), 158–175.

Rustichini, A. & Vostroknutov, A. (2014). Merit and justice: An experimental analysis of attitude to inequality. *PLoS ONE*, 9(12), e114512.

Tokumaru, N. (2014). Fairness ideals, hidden selfishness and opportunist behavior: An experimental approach. Kyoto University, Graduate School of Economics, Research Project Center Discussion Paper No. E-14-004.

Ubeda, P. (2014). The consistency of fairness rules: An experimental study. *Journal of Economic Psychology*, 41, 88–100.

Vostroknutov, A., Tobler, P. N., & Rustichini, A. (2012). Causes of social reward differences encoded in human brain. *Journal of Neurophysiology*, 107(5), 1403–1412.

Curriculum Vitae

Arnd Heinrich Klein

* 21.01.1985 in Saarbrücken (Germany)

09/2010 – 04/2015	Doctoral studies, Zurich Graduate School of Economics, University of Zurich (Switzerland)
02/2014 – 07/2014	Visiting Scholar, Haas School of Business, UC Berkeley (USA)
10/2005 – 07/2010	Diploma in Economics, University of Konstanz (Germany)
09/2008 – 09/2009	Master of Science in Economics, University of Oregon (USA)